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MIND, BODY, AND SOUL IN MEDICAL PSYCHOLOGY

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It was a red-letter day for early man when he learned in his simple logic how to pass from pairs to trinities, from the coupling of two comparable things to a grouping by threes. The simplicity and the natural conjunction of pairs are obvious. You either regard as coupled two distinct individuals (as a pair of oxen or a dancing pair), or you alternate complementaries (right or left hand, the to-and-fro of the pendulum), or, finally, you set *this* in opposition to *that* (as up against down, sweet against sour, love against hate). Much primitive logic is comprised under these various forms of pairing. This *and* that, this *or* that, this *against* that; all combinations easily spanned and readily dealt with as units.

Even so complicated a performance as social estimation is usually compassed by the simple logic of two similars or two opposites. Take any one of fifty epithets by which we label our neighbors, our lovers, and our enemies, and you invariably find that it has its apt opposite. An individual is good or bad, learned or ignorant, adorable or intolerable, frugal or thriftless, and so on with the entire list. The individual who is branded by the one excellence is removed as far as possible from its opposing vice. Primitive thinking —the kind most used to this day in making our social adjustments—likes best to deal in high light and shadow and not to bother with the intermediate tone.

But a logic of three is much more sophisticated. There you cannot simply relate *this* and *that;* you cannot pass comfortably back and forth, and you cannot stretch a similarity until you have reached maximal unlikeness, as when you remove the east from the west or oppose sin and virtue. Some new form of integration is required, something with a wider span than the pair. One of our neatest devices for setting threes in order is the triangle. A corner

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or a side suffices for each and for each in relation to the others. Small wonder that the triangle with its significant properties has become one of man's most useful instruments for carrying and preserving his trinities! And it is not surprising that he has reserved the subtler logic of the triad for some of his most cherished and significant groupings, for his fine arts, his trinitarian godhead, and the notion of his own engaging and baffling personality. The Christian's God is Father, Son, and Holy Ghost; creator, intercessor, and comforter; three offices in one being. And similarly man is himself body (physical mechanism), mind (thinker and doer), and soul (mysterious and eternal member).

Now it is not by chance that the medical arts should have treated man as a composite being. Called upon to heal before man had clearly triangulated himself, medicine wrestled with demons and spirits, matching its cunning to theirs, and seeking to purify and to sustain the essences of man. The sick instrument was body, stretched prone, broken, or distorted by pain, albeit a body moved by mysterious forces. The whole man was involved. The medicine man or doctor was at once wizard, healer, and minister to the tormented and possessed patient.

Simplifying and rationalizing its task, medicine sought gradually to free itself from responsibility to the soul or spirit. The doctrine of demoniac possession it came in time to desert, maintaining that its arts were to be directed to biological functions deranged by poisons and other pathogenic agents.

With the rejection of possession and of the soul as the guest and companion of the body, medicine would seem to have been on the way toward a like elimination of the mind. Anatomy lays bare the body: pathology displays the evidence within it of disorder and disease: physiology gives an account of the coöperative functions and operations of the body, either as running a healthy and undisturbed course or as deflected and disturbed by physical agents and bacterial invasions.

Now there is no doubt that a purely *somatic* medicine would be far simpler and far more satisfactory than a theory and art of cure which looked upon the patient as a being composed of earth and of immaterial essences. And medicine has valiantly tried from time to time to restrict itself to the earthly frame and to the bodily functions. But it has developed in the same cultural atmosphere as philosophy and as the natural sciences have; and it has preserved with them the common heritage of a double nature in man, half physical and half mental. The Cartesian doctrine of matter and mind, extension and inextension, is far more than a philosophical theory. It is a well-nigh universal way of regarding man; a way, to be sure, that may be disregarded by the experimental physicist, to whom the mental aspect of existence

may appear only in the passive human observer. The world to be studied and controlled is for him physical through and through and subject only to physical principles and laws. Mind is present as the knower, but not as an object of observation. At most it is a source of calculable errors or of the annoying personal equation. When the physicist came upon the Cartesian man outside his laboratory—in his emotional life, for example—the non-physical part could always be referred to religion, to magic, or to the vagaries of psychical research. Animals, too, could be dealt with by the naturalist, as organized and living bodies, actuated perhaps by innate forces or instincts, but essentially mechanistic in their mode of operation and behavior.

Outside of the physical sciences, however, man has always been an enigma. Desire, fear, lust, and disappointment have been real and actual to him even when he was well enough to go to the laboratory, to business, or to sin. And when he fell sick, his somatic and mechanistic world became shaky and inadequate. The miseries and incapacities of illness, not to speak of the realities of life and death, have inclined man toward a binarian, if not a trinitarian, view of himself.

Moreover, the doctor as well as the patient has found it difficult to be purely somatic, to ignore non-physical factors and agencies. The double-man of Descartes has persistently stood before him whether for study or for cure. Without doubt the establishment of a doctrine of disease based upon parasitical and bacterial agents has at times tended to support his somatic view and to encourage his belief in the adequacy of physical medication in all forms of disorder. And his reluctance to admit non-physical pathogenesis has been strengthened by the mechanistic corollary that only the physical could cause the physical, since matter and energy form a closed and self-actuating universe. Mind doubtless exists, but it could never take the place of a bacterial agent in causing disease or of a physical medicament in curing it. If a bad stomach grew worse under worry or a frail heart faltered under fear, that could only happen because worry and fear had their neurological correlates which were doing all the bad business which the untutored laymen laid to his uncomfortable feelings. The feelings as felt were only mental epiphenomena and could not therefore alter gastric juice or disturb cardiac contraction.

Nevertheless, it has long been obvious in a hard-headed view of things that cheerfulness and courage are better medicines than worry and fear, and that an unrelieved strain in business may well be followed by digestive upset, twitching muscles, and pasty skin. Many men, especially those who have been responsible for the disorders classified as neuroses, have therefore modified their somatic views and have added *psychogenic* causes. The or-

ganicist, however, refuses to be moved, maintaining that all diseases are really organic, those appearing to be 'functional' only hiding for a limited time their real bodily character.

You know, far better than I, how active is the contention of the parties, especially as they battle in the field of the mental diseases. The nervous stomach and the delicate heart may themselves raise the issue; but when the somatists face the extreme disorientation of the psychoses and the vagaries of the neurotic, they find it difficult to pacify the psychogenicists with the promise that complete and adequate bodily explanations will presently be at hand and that, in the meantime, pious hope is better than recourse to the psyche.

Within our own times the problem of origins in mental medicine has been further complicated by a return from the simple pairing of mind and body to trinitarian views. In certain quarters man is not just conscious organism, *i.e.*, body and mind, but body-consciousness-unconscious. Again we have the triangle with the unconscious written in capitals at one corner. In some forms of that doctrine which admits the unconscious to membership in the organism, the unweighted triangular form of the trinity is scarcely sufficient. The unconscious is really basal; it is, so to say, the father-and-creator. Unconsciousness is aboriginal. The organism only comes to existence when pushed by its libidinous urge or *id*. It is life-energy. Only gradually appear body and the ego to complete and to round out the total being.

Now this return to a triune being is not a merely formal and speculative proposal. In the doctrine called psychoanalytical, the adherents profess an understanding of disease in terms of an actual three-fold organism whose fundamental and elemental member is the unconscious. To the proposal that the mentally disordered be understood, explained, treated, and cured in the name of this trinitarian doctrine, medicine was at first indifferent, then scornful, then combative, then argumentative, then infected, then divided within her own house, then . . . but who knows the future?

Now I am no more interested than you are in theories and doctrines, in speculations and assumptions, in the tenets of schools or the debates of believer and skeptic. To embrace, to belong, to champion, and to decry are not human attitudes and functions that attract me. I am not here to urge or to combat one doctrinal position or another.

At the same time, I seem to see in all this current debate among the somatists and the mentalists, the organicists and the functionalists, the psychiatrists and the neurologists, and analysts and the anti-analysts, new turns to old contentions about the one and the many in the constitution of man, and new turns wherein the psychologist should accept some responsibility

and should offer—provided it is within his power—some service to the medical arts.

Were these conceptions of the dependence of disease upon the constitution of man merely historical views, remote in time and place, they might safely be left to the historian of medicine. But they are not. The single man, the double man, and the triple man are all present-day conceptions. Moreover, they determine present-day practices. It is not likely that they are all equally applicable to the same body of biological fact. If we should examine them dispassionately, without regard to their philosophical implications, we should discover—as I am quite sure—that each has its practical virtues and each its limitations and defects.

May we dwell upon them briefly and without contention?

(1) The first view regards man as diseased in his body, the disease referable to physical causes and agents, and curable (if at all) by material means. This view may admit conscious symptoms and it may also hold it to be possible to get at and to improve the bodily state by appealing to such conscious things as desire, mood, determination, and the like.

It is obvious that a large part of medical practice proceeds sensibly and with whatever success physical medicine actually attains, under this view. But it also appears that certain morbid states and conditions (notably some of the insanities, as well as the phobias, the impulsions, and the anxieties) are imperfectly understood and indifferently treated under the view described.

(2) The second view regards man as a paired double; as a creature composed of two complementary parts called body and mind; as a psychophysical or psychosomatic organism. As members or parts of the organism, the physical and the mental are coördinate, inextricably conjoined while life lasts. In one form this view has regarded the physical and the mental as different aspects of the same thing or substance (double-aspect theory); in another form the anatomical structures have been primary and the mental structures dependent upon them (dependence-theory); in still another form mind and body have been equally efficacious—albeit in different ways—in the conduct and the government of the organism (double-adaptation theory).

In one form or another this 'paired-double' view was generally sustained by most successful systems of psychology up to the opening of our century. It is obvious that it too may be, and has been, eminently useful in medicine. It has been especially popular whenever medicine has been inclined toward the biological view of adaptation and mal-adaptation, for here mind may be viewed as a well or a diseased instrument of adjustment.

In practice, medicine distinguishes physical diseases from mental diseases, though it has never given the term 'disease' precisely the same meaning in

the two contexts, and it usually hastens to explain that 'mental disease' also implies the sick body.

This double-aspect view usually persuades the practitioner that when he is concerned with mental diseases he is also a psychologist. As a result he frequently writes a 'psychology of the abnormal' or a 'psychology of mental disorders' which is a miscellany of medical facts and theories with terms and fragments thrown in from general psychologies, which are usually antiquated; these terms and fragments carefully selected to fit the favored medical theory. The result is not a psychology in the psychologist's sense, but a medical medley and theory of symptoms interspersed with psychological terms.

(3) The third view opens the door—as we have seen—to the unconscious. We think first of Freud's doctrine and practice. The fundamental contention here is that libido or some other primal urge is a permanent and enduring part of the living being, and that it resides in, or makes up, the unconscious. Certain temporary and conscious desires may be repressed and thus be transformed and transferred to the unconscious. Here they are active and dynamic, seeking to escape and to determine action. Conflict ensues. The organism and its functions are disturbed. If uncontrolled, disease may follow. Where disease thus originates, its relief obviously depends upon making peace with the unconscious. The prescribed method is psychoanalysis. Disorders of this sort are said to derive from a psychogenic origin; but not psychogenic in the meaning of the second, or mind-body, view. Psychogenic as involving the unconscious or (to employ a later Freudian term) the *id*. Their exorcism is said, therefore, to depend upon 'depth-psychology,' a term now treasured by many adherents of this doctrine. In Freud's hands, this view emphasized sex as the source of the unconscious thrust or libido. There it also led to a host of accessory concepts; repression, transference, complexes, symbolism, dream-work, sublimation, and many others. Less violent adherents to this trinitarian view have made it evident that some of these concepts are unnecessary; that other motives may replace the sexual, transference may be reduced to an empirical personal relation, and the naked soul itself drawn from its cavernous depths to the twilight of the subconscious.

Were we asked to name the chief advantages to medicine from such a doctrine, we should suggest that, in our view, the gain lies less in adding a third and hypothetical party to the human constitution than in quite another direction. To be sure, this third constituent is conceived as powerful enough to supply the main motive power to the organism, and disturbing enough to generate a good half of the ills that flesh is supposed—by the somatists—to be heir to. But it is not, as I think, this powerful and hypothetical agent

that most strongly sanctions and commends the psychoanalytical movement. It is, instead, the contention that disordered function has at times its source in the history of the organism, a history that is quite as truly private and biographical as it is racial and biological.

Wholly apart from this question of the psychogenic origin of disease, it has of late been made abundantly evident that many disturbances of the organism derive, at least in part, from earlier facts and incidents in the individual life. Shame, worry, remorse and fear, as well as unhealthful habits and practices of childhood, obviously have their untoward sequels in the later life. No other organism is so highly individualized in its life-course as is man. This unique position is chiefly due to human socialization, which makes the individual a member of a family, an active partner in many lesser and greater social congregates, and a center for local, folk, and social traditions. Man lives as much by social valuation and estimate as by the physico-chemical interchanges common to his kind.

Now this fact that biographical history counts as much—in adequate and in inadequate functions, in the healthy and defective living of men—as does the common biological sequence of growth, development, and decline has been stressed by the trinitarian, who is always insisting that cure must wait upon an analytical process which recovers, identifies, and estimates the biographical past. Whether or not we follow him in his assertions about the unconscious, the conflict and the symbol, is quite another matter. His speculative structure rests upon a bold and bizarre notion of the soul and of its libidinous nature, its innate greed for power, or its draughts upon the racial unconscious.

This is the trinitarian's mode of interpretation. But we must remember that biography may be conceived in wholly different and non-analytical terms. It may, for example, be written down in terms of genetic psychology, which first obtains samples of the life-course, taken directly from infancy, childhood, youth, and the several decades of adulthood. These samples may be drawn from many individuals, compared and described in longitudinal course as natural sequences in the individual life, as dependent upon stock, diet, education, family vicissitude, and many other factors. To reach the idiosyncrasies of the individual, this genetic method must, to be sure, call upon social psychology for principles of socialization of the individual, and it must look both forward and backward in the individual course to catch the long run of developmental tendencies in their actual sequence. In its backward glance, it may use *any* legitimate method of history; it is by no means constrained to adopt the speculations of the ego, the libido, the id, and the unconscious.

We must admit that, even in his most determined and reasonable thinking, man loves short-cuts. He will often tolerate the vagaries of a speculative scheme to escape the laborious collection and sober interpretation of facts. But to the patient and the industrious there is always available the empirical method of a sound genetic psychology and biology to take the place of an assumed unconscious soul which has been made to order to fit a speculative doctrine of origin and cause.

But let us return to our three men; our single man, our double man, and our triune man. Return to ask whether it is not possible by setting philosophical and medical tradition aside and by scrutinizing the human animal himself, to simplify our common problems.

The proposals which I shall have to make I have previously made for psychology;[1] but for psychology on the assumption that what is applicable within its general territory should be applicable also in all those special excursions which psychology has had to make in various directions; into the social field (social psychology), toward the study of children (child psychology), for the observation of other forms (animal psychology), the history of human varieties (anthropological and ethnological psychologies), and the study of human disorders (psychology of the abnormal).

It is a disturbing fact that there has never been a sound general and basal psychology which was also satisfactory in all these special fields as well. The result has been that the sociologists have made up their own sad social psychologies by borrowing from us a few resounding terms (such as predisposition, habit, group-mind, and social consciousness) to set off favorite sociological doctrines of their own; the biologists have generally picked out such doubtful concepts as instinct, reason, association, learning, and choice, to base their animal psychologies upon; while psychological medicine, psychiatry, and mental hygiene have either raked upon antiquated concepts from psychologies yellow with age (the commonest being that of instinctive powers) or else have given an air of modernity to their psychological writings by importing some such alleged novelty as psychoanalysis, behaviorism, or Russian conditioning, doctrines which have never yet been really acclimated in general psychology itself.

There is a certain excuse on both sides. These special jobs are comparatively new for psychology, and she has not yet learned how to do them well; and, on the other side, those seeking psychological material have not known just where and how to look. All the same, those persons turning away from psychology with only dry bones or with empty hands can well complain that "when they got there the cupboard was bare."

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[1] *Psychologies of 1930*, 1930, 95-115; *The New Field of Psychology*, Pt. i, *The Psychological Functions*, 1933, 1-12.

To be sure, our self-love is forced to add the consoling reflection that the borrowers have been much keener to take away from psychology some neat and striking bit of speculative doctrine easily applied than attested facts and principles. When they go abroad for help, the sociologist wants the 'original nature of man' wrapped in a two-ounce package, the educator asks for an easy formula for learning, the biologist seeks an adaptable mind that will explain survival under struggle, the advertiser wants a recipe for persuasion, and the medical man either adopts the ancient faculties of reason and of will, which are variously supposed to fall into disorder and disease, or else he adopts the more modern subconscious, the will to dominate, the libido, the Œdipus complex, or the phobia of castration.

But let us not waste time in the distribution of awards and of reproaches. Our main question lies directly before us. What is the best, *i.e.* the hardest-headed and the most useful, way of regarding man when we have to deal with him sick or well from the psychological side?

The *double man* has never been satisfactory either to you or to us. The hoary mind-body problem that he brings with him is a night-walking spectre that is never laid. Mind is obviously an efficacious thing; but how could it be? Mind seems to be diseased as well as body; but how—lacking tissues—can it be diseased? Mind should—since it is one-half of man—be described in laboratory and clinic. But how shall it be?

The *triple* man or human trinity is, like the divine trinity, a matter for acceptance and conviction. If you believe in the unconscious, libido, and the id, you will easily believe in a score of other alleged and accessory dynamic agents. If that is the sort of thing that you like, then it is likely that only a new conversion or a strict training in logic will make you dislike it and lead you to reject it as a doctrine unusable in medicine.

Where belief in this doctrine (I am not now speaking of the clinical *method*) has been incorporated into medicine, medicine has been threatened with a revolution, and I have observed that medicine is not fond of revolutions. At present medicine has no machinery for controlling such an innovation and it is not fond of practices and doctrines which it cannot control, which it must share with the layman, with a religion, or with the unauthorized practitioner. I do not intimate, of course, that many doctors of medicine of sound training and in good repute do not usefully employ psychoanalytical concepts and practices, sometimes, as it appears, to their great advantage and to the well-being of their patients. That is another matter. I speak only of the incorporation of the three-fold man into organized medicine; of man with a hidden soul as well as a revealed body and an open mind.

Let us look, then, at our proposed revision of the *single man*. The man who is a body. Ah, then—you may say—we come back to behaviorism, a modern

and respectable view, a standpoint sometimes taken in psychiatry and supported in psychiatrical practice!

Now there are several brands of behaviorism, and I do not mean to afflict you by discussing and distinguishing them; though I must remind you that it is one thing to enter upon the serious and scientific study of bodily performance for its own sake and quite another to maintain that such a behavioral study is adequate to the knowledge of man, sick or well, of his conduct, and of his springs of action. Psychology engaged in behavioral studies long before the doctrine of behaviorism, advanced as a substitute for psychology, was successfully advertised.

It has been obvious to psychologists as long as experimental methods have supported them that 'objective' or behavioral methods have had a wide and distinctive use; but all except a few violent enthusiasts have always seen that many problems of human living, normal and abnormal, have never been successfully formulated, not to say solved, by this type of method, and that the addition of a creed of behaviorism did not really change the nature of man or alter the face of natural facts.

But behaviorism does not offer the only alluring view of the single man. There is at least one other view. To apprehend it, we should again try to forget all theories, doctrines, and hypotheses regarding the nature of man and to consider him as he concretely is. To this end we should follow him in his daily living. Let us, *e.g.* examine the specimens of him now in this room. We are sitting or standing by virtue of certain postural functions. Respiration, circulation, digestion, and the like are going on. Given my time and your knowledge, we could make out a tolerably comprehensive list of our common physiological functions now in course. But would that list exhaust our present activities? No. The most obvious (and possibly the most difficult) thing that you are doing is understanding. I speak and you listen. Nothing is more obvious. But search the biological sciences (anatomy, physiology, embryology, ecology, behaviorism, and all the others) as long as you will and you will find no adequate description of the hard fact that I address you and that you see me, hear me, understand me, and critically value my words.

Say all you will about receptors, nerve impulses, action currents, cerebral processes, and muscular contractions, and you will not describe (I do not say 'explain') these obvious facts. Declare in addition, if you will, that you are all responding to visual and auditory stimuli supplied by the room and by me. Still you have no more touched the central fact than the astronomer who should seek to describe a galaxy by saying that suns and planets stimulate and respond to one another. In more general terms we may say that

there is a whole group of psychological functions—understanding, perceiving, remembering, thinking, emotion, and the like—that must be *added to the physiological functions* of which I spoke a moment ago. As functions of the living organism, neither group can be resolved into the other, and for an adequate description of human activity the psychological functions are just as necessary as the physiological functions are. But the psychological functions by no means imply 'mind.' There literally *is*—in our present view—*no mind,* in the sense of a mental kind of existence. Anatomy and morphology are the only fundamental sciences of existence so far as the living organism is concerned. When we say 'is' we should mean the integrated body. Naïve man added the fiction of a soul-mind because he thought that he should have some sort of enduring self that the brutes did not possess and that, moreover, would help him to understand many puzzling things about life. Sophisticated man followed and strengthened the naïve tradition and made it more circumstantial. Later, reflective man came to distinguish two kinds within the fiction of the soul-mind, the conscious mind annexed to the body and the soul behind it, which appears in the present preferred forms of libido, *élan vital,* entelechy, and the like.

When he is dressed in his working clothes and observes with the training of the laboratory behind him, man does not *observe* mind and he does not *observe* soul. What we all observe, structurally speaking, is the body, and what we all observe, functionally speaking, are the physiological and the psychological functions. When objects are announced to the living organism they are announced by way of perception, memory, or imagination—all announcing or apprehending functions; for thus the organism lays-hold-of, seizes, or apprehends objects and events. When predicaments arise they arise by way of the emotive functions. When thought-problems proceed toward solution they always proceed through the elaborative or thinking functions. All these functions are modes of activity not comprised within the list of operations studied by the physiologist.

But when we ask *how* these things are done—how we perceive, understand, think, and the rest—we can only answer that the *body does them.* We never observe the mind doing them. Mind is a fiction created to make an easy explanation, and when that fails we create some form of soul. Both entities are of great value for literature, religion, music, and other aspects of the *private and the romantic life* but are without a place in the sciences.

Sherrington somewhere remarks that we have no adequate notion of the variety of things which the body can do until we dismiss our prepossessions and observe it actually at work. As a matter of hard fact, we lose nothing for scientific description when we lose mind. The conquests of human

knowledge, the amazing deposits of social tradition, the formulation of laws, and the creation of fine art and letters lose none of their dignity and none of their value to man when we discover that they are products of bodily structures, wrought out through the agency of functions which we may conveniently call 'psychological.' We can no more conceive social organization or poetry or human tradition as coming into existence without the mediation of the psychological functions than we can conceive the living cell without metabolism or a building without the operations of mason and carpenter. But knowledge and art, custom and law, appear to derive just as directly from the properties and the capacities of the integrated organism as do the physical products of gland and muscle, of blood and viscus. No more do we find it necessary to assume the mysterious existence of mind and soul than of vital fluid and animal spirits.

But we must also regard these psychological products in their own character if we would understand the psychological operations behind them. We must regard them not only because product always throws light upon function—as secretions and excretions of the body upon its physiological operations—but still more because the organism is constantly exposed to its psychological products and (in man, at least) its life-course is largely determined by them.

Some men have tried to persuade us that the physical order is only our percept, a product of our apprehension. That may be left to the philosophers. Our interest lies in the fact that all psychological products throw light upon their organic origins. They also form, so to say, the field within which the organism continues to operate. Especially do the social products of the organism. *i.e.* social organization, tradition, opinion, tabu, institutions, and morals, react upon the organism to temper those functions which made them and so direct and determine the subsequent life-course. Man proceeds through life, therefore, as through a sea of socialized objects, events, and occasions which his own functions have created. Since he always proceeds from infancy to death as a particular organism, reacting in a particular and individualized way to this fluid medium, his life becomes a personal biography and not merely a biological history of a pattern common to all men.

Now one more corollary of the functional view and we shall have everything necessary to a psychology of disorder. That corollary proceeds from the fact that function itself changes the organism. It changes it by disposing the organism toward future performance. When renewed, functions tend to repeat themselves in the old way. This is the functional fact of trend or predisposition. Every organism, *i.e.* living body, is the scene of a constant stream of functions all molding the organism toward future modes of opera-

tion. This means that no psychological function is wholly new or naïve. It has other functions behind it. This is the past active in the present. The body thus carries functional trends by the thousand, some general (as in right-handed performance or in a tendency toward sentimental effusiveness) and some specific (as the memorial recovery of a former event). How the body continues to hold these residues of earlier function we do not know. That it does we have abundant evidence both in physiology and in psychology. The past, without living again, is nevertheless able to color, determine, or govern the present.

Life therefore is cumulative in this functional sense. Its process is irreversible. And since its temper and direction are determined, first, by the specific character of an individual past, and second, by the specific reaction of specific products upon the organism, every life-course is bound to be individual and biographical.

In fine, then, we have a living body apt in many occupations and performing in many diverse ways and by many modes of activity, which we segregate into two great classes, physiological and psychological. The first mainly tend toward integrating the body in an active way and in interchanges between outside and inside materials and energies. The second mainly tend to make the organism "an active agent in *apprehending* itself and other objects, in *initiating* and prophetically *carrying out* its own activities, in *setting and resolving* problems, in *divining* the significance of things, in *communicating* and *participating* with others of its kind in the affairs of human groups, and in *combining* its beliefs, customs and knowledge into large organized systems. Where the physiological functions constantly fashion and reconstruct the living body, the psychological functions fashion and reconstruct knowledge, conduct, society, and the universal theatre of objects, events, desires, and values. For the one kind of function the living body is a *vehicle,* for the other it is an *agent.*"[2]

Every sort of function has its issues—the physiological sort turning out secretions, delivering oxygen, expelling waste, building new tissues and the like; the psychological sort fashioning objects, creating knowledge, crystallizing traditions, forming laws, and setting up values.

Some physiological products immediately serve the functions themselves. Witness hormones and such substances as adrenin and thyroxin. Others store fuel and energy against future use. Some psychological functions make tools for man. Others make a social medium in which men live.

As life goes on its course is constantly made and revised as the functions

[2] M. Bentley, *The New Field of Psychology,* Pt. i, 1933, 7.

remodel and extend themselves through repetition and interaction and as the functional products return upon the organism to guide, govern, or afflict it. Knowledge is a psychological product which supports and directs the activities of the organism. Social prohibitions and tabus are products which obstruct desire and appetite, causing the type of emotive function which we call 'conflict.' One motive to conflict may lie behind, deriving from the persisting and cumulative bodily trends of the individual past; the other motive may lie without, deriving from those products which are deposited in social tradition and formulated command. Neither element of conflict lies in the mysterious depths of a hypothetical unconscious or in a hypothetical and persistent libido. The body on one side supports its functional trends and its persistent aptitudes; psychological products of previous functioning, on the other, reënforce and redirect the course of life. For the psychologist, at least, there would seem to be no necessity either for mind or for soul.

Were the medical and psychiatrical arts to come seeking a fundamental view of human living and of human derangement, they would find in this empirical view of things a single, and more or less unitary, organism which is structurally a living body and functionally a seat of many operations. Both sorts of function it would find frequently deranged, disordered, inadequate and maladjusted. Its first task would be to describe and to understand the functional defect, its second to search out all those factors present and past which bear upon the derangement, its third to discover the bodily resources of function, both for its normal and its defective course, and, finally, to seek means and medicaments of relief.

The distinction between 'functional' and 'organic' as classes of disease would therefore vanish under this approach, for we should find always functional disturbance and always the bodily and extra-bodily ground for the disordered, as well as for the healthy, function.

A revision of the concept of 'mental disease' might well be considered, first because 'mental' (as implying 'mind') would be superfluous, and again because disordered function would by no means imply either a lesion or a psychogenic agent afflicting the body. Just as disorder in the running of the motor car often implies poor gasoline, carbon-deposit or defective lubrication, but not an *engine-disease,* so the deficiencies and the defects of schizophrenia and of the neuroses do not necessarily point to lesions, invading organism or pathological states of mind, but may instead lead the diagnostician to a blocking or stasis as between functional patterns or to an unwholesome descent upon the organism of afflicting and devastating products of antecedent function—our own antecedent function and the function of many another organism as well. The organism will have to be re-

garded less as an invariant biological sample of *homo* and more as an individual biography, constantly changing in time, and interacting with other biographies in individual ways and also with the great social deposits of our race and time.

Those bodily changes which directly underlie and outlive function will, at times, still be difficult to find—their full discovery may await new physiological and biochemical methods which we now only glimpse; but a persistent search for actualities of operation and of structure will be far better than a flight to fancied hereditary causes, to the gross paradigms of symptomatology, or to vagaries of 'mental symptom' and of 'unconscious cause.'

To the psychologist, of course, the concept of 'disease' is foreign, even to the psychologist of the abnormal. It follows therefrom that 'symptoms' and 'diagnosis' are not for him. Both imply disease. The offices of the diagnostician and of the physician are not his. The psychologist deals descriptively with his functions, with their bodily antecedents and mechanisms, with their history, and with their products. Defective and inadequate functions are as much his concern as normal and adequate functions are. This would seem to imply a fair division of labor as between psychologist and physician, and, as we may hope, a useful and amicable form of reciprocity.

THE PSYCHOLOGICAL SIGNIFICANCE OF THE HOROPTER

By ALBERT DOUGLAS GLANVILLE, Cornell University

The determination of the horopter, that is, of the locus of the points in space where, with a given fixation of the eyes, an object will be imaged upon corresponding retinal points, has long constituted an important problem in binocular vision. Since no one has ever made a universally acceptable determination of the horopter the problem still interests students of psychology and of physiological optics.

Nearly all of the early attempts to determine the horopter grew out of theoretical, mathematical, or anatomical conceptions of corresponding retinal points. Many of the later experimental determinations were based in part upon more or less casual observations of a single observer. In some instances these determinations were invalidated by structural peculiarities of the observer's eyes, and more frequently the conclusions drawn were distorted by a theoretical bias of the investigator. In the course of the last three or four decades a few sound experimental studies have been made; but, because of the uncertainties of peripheral vision, the observers have generally been instructed to report in accordance with some assumed criterion of retinal correspondence rather than to report whether the object was seen as single or as double. The outcome of these investigations has been, when the existence of the horopter was not denied, a variety of proposed horopters, none of which has proved to be generally acceptable.

The present study was undertaken for the purpose of determining the points in space at which, with a given fixation of the eyes in the primary position, a simple object will be *seen* as single. For the sake of making a fresh attack upon the problem, it has seemed desirable to leave aside the various intricate notions concerning retinal correspondence as well as the previously determined horopters and to attempt to determine an *empirical* horopter upon the basis of results obtained from observers who were instructed to report upon the precise character of peripherally perceived objects.

HISTORICAL SUMMARY

The literature on the horopter and on the closely related problems of retinal correspondence and of binocular single and double vision is so

* Accepted for publication May 20, 1932. The study was carried out in the Psychological Laboratories of Cornell University under the direction of Professor Bentley.

voluminous that it would be inexpedient to give here anything approaching a complete summary of it. Nothing more will be attempted at this time, therefore, than to review briefly some of the more significant studies of the horopter and of related problems.[1]

Aguilonius, a Jesuit writer on optics, seems to have first used the term *horopter*. In his *Opticorum libri sex*[2] (Antwerp, 1613), he proposed the theory that the horopter is a plane passing through the fixation-point perpendicular to the median line of sight. He argued that the images from the two eyes were projected upon this plane and that only objects seen in this plane appeared single.

A markedly different form of the horopter was proposed about two centuries later by Vieth and J. Müller. Vieth,[3] in 1818, said that the visual plane cuts the horopter in a circle passing through the fixation-point and the centers of the lenses of the two eyes. Objects located upon this horopter should be seen as single, he contended, because their images would fall upon corresponding retinal points, whereas objects at other positions would produce images upon non-corresponding points and should therefore appear double. J. Müller,[4] writing a few years later, regarded corresponding retinal points as being anatomically fixed points which were located equidistant and in the same direction from the centers of the two retinas. Although he thought that the complete horopter should be a surface, Müller concerned himself with only the part of it passing through the visual plane. In accordance with his notion of the location of corresponding points, he showed that this horopter is a circle which is identical with that of Vieth. This circle is now usually called either the Müllerian horopteric circle or the Vieth-Müllerian circle.

In 1838 Wheatstone[5] attacked the validity of the theory of corresponding points and showed the inadequacies of the horopteric plane (Aguilonius) and of the Müllerian horopteric circle. Upon the basis of observations made with the aid of the stereoscope, which he invented, he demonstrated that objects not in the horopteric plane are very often seen, not double, as Aguilonius had argued that they must be, but single. Likewise, he demonstrated that objects whose images fall upon disparate retinal points may be seen single, thereby proving that objects not falling in the horopteric circle may be seen single. Furthermore, he found that under some conditions objects whose retinal images fall upon corresponding points

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[1] Brief summaries of the earlier writings may be found in H. Helmholtz, *Hdbh. der physiol. Optik*, III, (3 ed.), 1910, 394-396, and in E. Claparède, Quelques mots sur la vision binoculaire et stéréoscopique et sur la question de l'horoptre, *Arch. sci. phys. et nat.*, 3, 1853, 138-168. For a summary and discussion of the more recent experimental studies see A. Tschermak, Optischer Raumsinn, in *Hdbh. der norm. u. pathol. Physiol.*, 12, 1931, 891-916.

[2] The writer was unable to consult a copy of this rare work, but he has found it referred to by H. Helmholtz's *Handbuch* (3, 1910, 394) and by other reliable writers.

[3] G. U. A. Vieth, Ueber die Richtung der Augen, (Gilberts) *Ann. d. Physik u. Chem.*, 58, 1818, 233-251.

[4] J. Müller, *Zur vergleichenden Physiologie des Gesichtssinnes*, 1826, 71-83. See also his *Handbuch der Physiologie*, 2, 376-387.

[5] C. Wheatstone, Contributions to the physiology of vision, *Phil. Trans. Roy. Soc. Lond.*, 1838, pt. 2, 371-394.

may be seen double. From this latter observation he argued that it was an error to assume a necessary physiological connection between corresponding points. He also suggested that the theory of corresponding points was an unsatisfactory explanation of binocular single vision. These observations and arguments have been severely attacked by several writers who followed Wheatstone.

On the basis of the theory of corresponding points A. Prévost[6] demonstrated the theory that the horopter consists not only of the Müllerian circle but also of a vertical line passing through the fixation-point.

Meissner[7] was the first to attempt an experimental determination of the horopter. He constructed a simple apparatus by means of which threads could be held in various positions so that an observer could report where the threads appeared single and where double. He recognized the necessity of making a large number of observations and of controlling movements of the head and eyes. From his results he drew the following conclusions: (1) When the eyes are in the primary position and the fixation-point is at an infinite distance, the horopter is a plane perpendicular to the median line of sight. (2) When the fixation-point is at some nearer position in the primary visual plane, the horopter is a line inclined toward the visual plane and dipping toward the observer, the inclination increasing as the fixation-point is brought nearer to the observer. (3) When the visual plane is turned upward, the inclination of the horopteric line increases, whereas when the visual plane is turned downward, the inclination of the horopteric line decreases until it becomes zero at 45° and then expands into a plane passing through the fixation-point perpendicular to the median line of sight. Meissner related these results to a rotation of the eyes outward in such a way that the vertical lines of demarcation coincide with the vertical meridians only when the eyes are fixated upon a point at infinite distance.

In 1858 Claparède[8] published a series of articles on binocular vision and the horopter in which he critically surveyed the earlier literature and then went on to propose a horopter based partly upon observations which he had made and partly upon theory and geometry. According to him the horopter is a complex surface which contains a vertical line passing through the fixation-point (the horopteric vertical), the Müllerian circle, and all circles passing through the nodal points of the two eyes and tangent to the horopteric vertical.

In a monograph on binocular vision Panum[9] proposed a modification of the theory of retinal correspondence which later played an important part in some of the explanations advanced to account for the fact that objects not on the Müllerian circle are not necessarily seen double. Observations which he made showed, as Wheatstone similarly had found, that an object whose retinal images fall upon somewhat disparate points might still be seen single. Panum supposed that a point a on one retina might be identical not only with point a' on the other

[6] A. Prévost, *Essai sur la théorie de la vision binoculaire,* 1843.

[7] G. Meissner, *Beiträge zur Physiologie des Sehorgans,* 1854, 1-121.

[8] E. Claparède, Quelques mots sur la vision binoculaire et stéréoscopique et sur l'horoptre, *Arch. sci. phys. et nat.,* 3, 1858, 138-168; Nouvelles recherches sur l'horoptre, *Arch. sci. phys. et nat.,* 3, 1858, 225-267; Encore un mot sur l'horoptre, *Arch. sci. phys. et nat.,* 3, 1858, 362-368.

[9] P. L. Panum, *Physiologische Untersuchungen über das Sehen mit zwei Augen,* 1858, 52 ff.

retina but also with a number of adjacent points in a circular area around d'. He called these circles "corresponding circles of sensation." According to this theory, then, the image on point a might be fused with the image on any point in the corresponding circle of sensation in the other eye. This marked a decided change away from the notion of anatomical correspondence in the direction of physiological or functional correspondence.

We come now to the consideration of the elaborate theories of the horopter worked out at about the same time by Hering and Helmholtz. To understand these theories in detail requires an extensive knowledge not only of the explanations of the perception of space advanced by each of these two men but also of physiological optics and of mathematics. We do not undertake to give more than a very general summary of these theories.

Before taking up Hering's[10] determination of the horopter we must first mention certain significant aspects of his notion of corresponding points. He assumes that when individual points of the retina are stimulated we have aroused, in addition to sensations of color, a feeling of direction and a feeling of depth. The feeling of depth is supposed to have equal and opposite values for each pair of corresponding points, but for any pair of retinal points which are symmetrically situated these values are equal and of the same sign. The feeling of depth of the temporal halves of the two retinas is positive and corresponds to an increase in depth; that of the two nasal halves is negative and corresponds to a decrease in depth. According to Hering, when two impressions on corresponding points are fused the total impression has the mean value of both the feeling of direction and the feeling of depth. Hering determines the form of the horopter in accordance with this theory, but we need not here follow his intricate reasoning; but we give his formulations of the horopter for near and for far fixation.

When the lines of sight are symmetrically parallel, that is, when a point at an infinite distance is fixated, the horopter includes practically the entire field of vision beyond a short distance from the eyes. Hering calls this a horizontal horopter. When the eyes are converged upon some nearer point there is a longitudinal horopter which is a cylindrical surface composed of a straight line perpendicular to the visual plane and which falls upon corresponding longitudinal sections of the two eyes. This horopter contains the Müllerian circle and a straight line passing through the fixation-point and perpendicular to it. Hering states that objects on the longitudinal horopter should appear as if they were in a plane perpendicular to the line of fixation. He calls this plane the *Kernfläche*.

Helmholtz's[11] determinations of the horopter are based upon the assumption that the eyes move in accordance with Listing's law of ocular movements, that the horizontal retinal meridians are coincident with the visual plane when the eyes are in the primary position, and that the apparently vertical retinal meridians are inclined so that, if one retina were placed over the other with their horizontal

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[10] E. Hering, *Beiträge zur Physiologie*, 1, 1861, 20-64; 2, 1862, 81-170; 3, 1863, 171-224; 4, 1864, 225-247; 5, 1864, 287-358. Also his chapter on "Der Raumsinn und die Bewegungen des Auges" in Hermann's *Hdbh. der Physiol.*, 1879, esp. 375-379.

[11] Helmholtz, Über die normalen Bewegungen des menschlichen Auges, *Arch. f. Ophth.*, 9, 1863, 188-190; Über den Horopter, *ibid.*, 10, 1864, 1-60; *Hdbh. der physiol. Optik.*, III (3 ed.), 330-393.

meridians coincident, the apparently vertical meridians would cross each other like an X, making an angle of about 2½°. Later writers, since they have usually found no evidence of this inclination in normal eyes, have presumed that it must be a defect of Helmholtz's eyes. It plays no small part in his views on the horopter, however, since he thought that corresponding points were determined in accordance with this inclination.

Helmholtz distinguishes between a point-horopter and a line-horopter. The point-horopter is the locus of points which will be seen single, and the line-horopter is the locus of straight lines which will be seen single in binocular vision. When the lines of regard are horizontal and converged upon a near fixation-point, the point-horopter is composed of the Müllerian circle and the horopteric vertical through the fixation-point. When the fixation-point is in the median plane and at infinite distance, the point-horopter is a surface. In this instance the horopter is a plane parallel to and below the visual plane: it is the ground on which the observer stands. Helmholtz devises two kinds of line-horopters, a vertical and a horizontal horopter. The vertical horopter is the locus of lines seen single which are apparently perpendicular to the retinal horizons, whereas the horizontal horopter is the locus of lines seen single which appear to be parallel to the retinal horizons. Since the forms of these vertical and horizontal horopters for various positions and distances of the fixation-point are rather complex geometrical constructions, we shall not include them here.

Helmholtz states that the problem of the horopter is a purely mathematical exercise involving a knowledge of the movements of the eye. He did make a number of simple experimental observations, however, to bear out his mathematical and geometrical demonstrations.[12]

Toward the end of the last century Hillebrand[13] undertook experimental work to test an hypothesis which Hering had previously made; namely, that objects located on the longitudinal horopter would appear as if they were in a plane perpendicular to the median line of fixation (*i.e.* in the *Kernfläche*). Earlier observations had led Hering to presume that the point on the nasal half of one retina which corresponds *functionally* to a point on the temporal half of the other retina, and vice versa, would not be at exactly the same distance from the center of the first retina as the other point would be from the center of the second retina. He suggested, therefore, that the 'empirical' or 'physiological' horopter would not coincide exactly with the mathematically determined horopter, for which it is assumed that corresponding points are at equal distances from the centers of the retinas. It was on the 'empirical longitudinal horopter' that he assumed that objects would appear to be in the *Kernfläche*. Hillebrand determined by means of the haploscope, an apparatus for holding three vertical threads in various positions relative to each other and in various parts of the visual field, the positions in which the three threads at small eccentricities from the fixation-point appeared to fall in the above-mentioned apparent plane. He found that these positions fell in a curve which deviated somewhat from the mathematical horopter. Since then this deviation has been referred to as the Hering-Hillebrand horopter-deviation. Hillebrand also con-

[12] Helmholtz, *op. cit.*, 395.

[13] F. Hillebrand, Die Stabilität der Raumwerte auf der Netzhaut, *Zsch. f. Psychol.*, 5, 1893, 1-59.

cluded from his results that fixed, definite pairs of retinal points give the impression of an object lying in the *Kernfläche* and that this relation is independent of the degree of the convergence of the eyes or of the distance of the object. This conclusion fitted in with Hering's nativistic theory of space perception and contradicted Helmholtz's notion that, as a result of past experience, different pairs of retinal points would give the impression of objects lying in the plane in question when the distance of the fixation-point was changed.

Accepting as an established fact Hering's assumption that objects lying on the longitudinal horopter are localized as being in a plane perpendicular to the line of sight, Tschermak and Kiribuchi[14] undertook to determine the longitudinal horopter experimentally, by finding the places where vertical lines appeared to be in the plane. They made two sets of determinations, one for plumb lines which were permanently in the field of view and another set for falling marbles. They found that the curve for the falling marbles (the drop-test horopter) was more concave towards the observer than the curve for the plumb-lines (the plumb-line horopter). Von Kries[15] later insisted that this was an investigation of depth-localization rather than a determination of the horopter. He argued that Tschermak and Kiribuchi described their results, not according to what they directly observed, but according to what they interpreted the results to mean on the basis of the assumption which they had accepted.

In 1907 Savage[16] criticized Helmholtz's determination of the horopter upon the grounds that it was based upon faulty optical constructions. Savage declared that Helmholtz had incorrectly placed the poles of the eyes and that he had incorrectly constructed the visual axes. He maintained, furthermore, that a line of vision is not an axial ray of light, as Helmholtz had said, but that it is a radius of retinal curvature prolonged. Savage then proceeded to demonstrate that the Müllerian circle passing through the fixation-point and the nodal points of the two eyes is not the true circle of single binocular vision but that the horopter is an isogonal circle passing through the fixation-point and the centers of retinal curvature which are the centers of rotation of the eyes. He also maintained that corresponding retinal points are points which have a common connection in the brain.

In 1924 Tschermak[17] and his student, Fischer, published a series of articles reporting results of experiments they had undertaken to determine the deviations of the empirical horopter from the Vieth-Müllerian circle under different conditions.

In the first of these articles, Tschermak[18] is concerned with a consideration of the so-called 'red' and 'blue' horopters. Plumb lines were suspended at various positions in the observer's field of view by means of the Hering-Tschermak apparatus. The eccentric positions were determined where the plumb line appeared

[14] A. Tschermak and Dr. Kiribuchi, Beitrag zur Lehre vom Längs-horopter, *Arch. f. d. ges. Physiol.*, 81, 1900, 328-348.

[15] In an editor's note by J. von Kries in H. Helmholtz, *Hdbh. der physiol. Optik*, III, (3 ed.), 1910, 315.

[16] G. S. Savage, A further study of the so-called horopter making ocular rotations easy of understanding, *Ophth. Rec.*, 16, 1907, 571-584.

[17] A. Tschermak, Fortgesetzte Studien über Binokularsehen, *Arch. f. d. ges. Physiol.*, 204, 1924, 177-265. The title given in this reference is the general title for the three articles. The specific titles for the individual articles will be given below.

[18] Tschermak, Über Farbenstereoskopie, *Arch. f. d. ges. Physiol.*, 204, 1924, 177-202.

to be in a plane through the fixation-point and parallel to the median line of regard. Red plumb-lines were used in one set of determinations and blue plumb-lines in another set. In both instances the horopteric curves obtained were flatter; *i.e.* they fell outside of the Vieth-Müllerian circle. The curve for the red plumb-lines was somewhat flatter than that for the blue plumb-lines. Tschermak explains this latter result in terms of the chromatic aberration of the eye. If, for a given accommodation of the eyes, red and blue objects are to fall on corresponding points they must be at slightly different distances.

Fischer[19], using the same method, verified Tschermak in regard to the red and blue horopters. He also found that the horopters for white plumb-lines on a black background and for black plumb-lines on a white background fall outside the Vieth-Müllerian circle. His results show that there is often a decided asymmetry in the shape of the empirical horopter-curves. For example, the curve for white plumb-lines may lie partly behind the geometrical fronto-parallel plane (the plane through the fixation-point and parallel to the line connecting the centers of the two eyes), on one side of the fixation-point, and entirely in front of it on the other side of the fixation-point. Fischer interprets this as indicating the existence of asymmetrical anomalies in the structure of the retinas. He also investigated the effect of length of exposure-time upon the shape of the empirical horopteric curve. The results of this part of the investigation confirm the earlier results obtained by Tschermak and Kiribuchi when they employed plumb-lines permanently in the field of view and also falling marbles. As the exposure-time is shortened the horopteric curve becomes sharper. Generally the empirical horopteric curve falls outside the Vieth-Müllerian circle, but with an exposure time as short as 0.2 sec. the empirical curve may fall within this circle. The 'white' horopter is flatter than the 'black' horopter, both for long and for short exposure-times.

In the third study of this series Fischer[20] adopted a new method of experimentation and repeated much of the work which we have just summarized. Previously he had made his determinations of the horopter upon the basis of the criterion of the apparent fronto-parallel plane, but in this investigation he employed the criterion of common visual direction. A brief description of the method of experimentation will indicate the meaning of this criterion. A peripherally seen plumb-line was so arranged that a small screen cut off the median part of it from one eye. When the retinal images of this plumb-line fell on corresponding points, the plumb-line was seen as one continuous vertical line; but when it did not fall on corresponding points, the median part of it which was hidden from the eye was seen as being either to the right or to the left of the rest of the line. The problem was to find for different eccentricities from the fixation-point where the plumb-line must be in order to be seen as one continuous vertical line.

Using this new experimental method, which Tschermak called the *Nonius* method, Fischer corroborated the earlier results for black plumb-lines upon a white background and for red and blue plumb-lines upon a black background. He did

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[19] F. P. Fischer, Über Asymmetrien des Gesichtsinnes, speziell des Raumsinnes beider Augen, *ibid.*, 204, 1924, 203-233.

[20] F. P. Fischer, Experimentelle Beiträge zum Begriff der Sehrichtungsgemeinschaft der Netzhäute auf Grund der binokularen Noniusmethode, *ibid.*, 204, 1924, 233-260.

not, however, find that the white horopter, determined according to the criterion of the apparent plane, corresponded to that determined according to the criterion of common visual direction. Since the latter criterion seemed to be the better, he concluded that the white horopter cannot be accurately determined by the older method. With exposure times of less than 0.8 sec., Fischer found that neither the white nor the black horopter was as sharply concave as the previous determinations had made them. It seemed, therefore, that the criterion of the apparent plane can be employed as a basis for empirical determinations of the horopter only when relatively long exposure-times are used.

Jaensch and a number of his students[21] have recently made many studies of the so-called horopter-deviations in connection with their investigation of eidetic and non-eidetic types. They have found a correlation between the observer's type and the amount of his horopter-deviation as measured in terms of the criterion of the apparent plane.

This brief survey does not catalogue all of the conflicting views in regard to the horopter which have been held and many of which are still held, but it does indicate that the problem of the horopter has not yet been settled. It also indicates that since most of the determinations of the horopter have been made from a mathematical or physiological point of view, we have as yet no direct answer to the question where, with a given fixation of the eyes, objects in the visual field will be *seen* single. It seems to us, therefore, that a more strictly psychological approach to the problem is desirable.

The Experiments

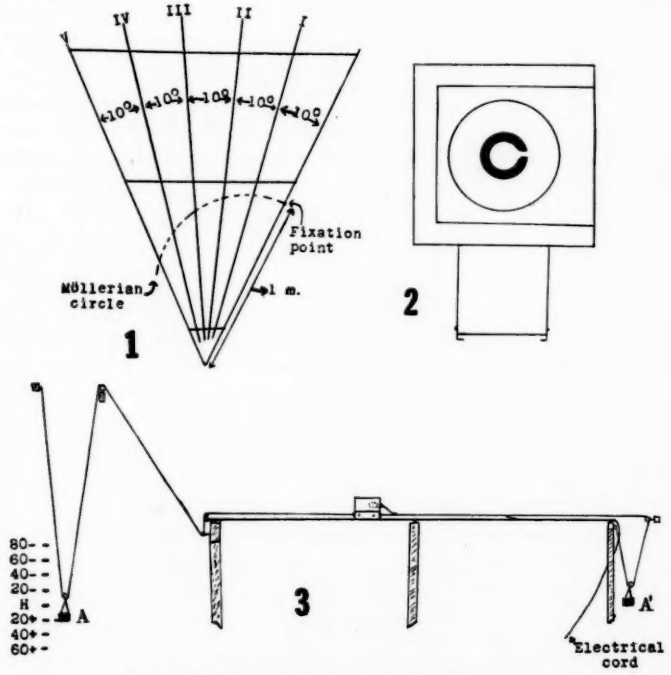
The experiments which we are to report may be divided into two groups: (1) those in which the *O*s were instructed to describe as fully as possible what they saw when a stimulus-object was presented in the peripheral field of vision; and (2) those in which the *O*s were instructed to report merely whether the object was seen as single or as double. The experiments belonging to the latter group were quantitative in nature, and they were preceded by two series of descriptive experiments in order to train the *O*s to discriminate carefully between the singleness and doubleness of peripherally perceived objects. They were also designed to furnish us with information about the adequacy of peripheral perception under the conditions of our experiment, so that we could use this information in interpreting the re-

[21] See, *e.g.* E. R. Jaensch and F. Reich, Über die Lokalisation in Sehraum, in E. R. Jaensch, *Über den Aufbau der Wahrnehmungswelt,* 1923, 59-67; K. Krönke, Zur Phänomenologie der Kernfläche des Sehraums, *ibid.,* 137-148; H. Ruschmann, Über das Verhalten der Wahrnehmungen jugendlicher Eidetiker unter Störungsreiz, *Zsch. f. Psychol.,* 1930, *Erg. bd.* 16, 41-51; E. Mayer, Die Funktionsschichten der räumlichen Wahrnehmung, *ibid.,* 73-76; F. Kranz, Experimentell-strukturpsychologische Untersuchungen über die Abhängigkeit der Wahrnehmungswelt vom persönlichkeitstypus, *ibid.,* 119-123.

sults of the quantitative series. The results of the quantitative investigation showed, however, that it was advisable to follow with another descriptive series. The experiments will now be reported in the order in which they were undertaken.

SERIES 1

Apparatus. The apparatus employed consisted of 6 horizontal steel tracks, each 78 in. long and 1 in. wide, along which light-boxes with a stimulus-object in the front side could be moved. One of the tracks extended straight ahead below and parallel to *O*'s median line of sight when the eyes were in the primary position, while the other five tracks diverged outward to the left at 10° intervals from one another. Each of these divergent tracks was the radius of an imaginary circle whose center was the point half-way between the nodal points of the two eyes. (See Fig. 1.)



FIGS. 1-3. SHOWING DETAILS OF THE APPARATUS

Fig. 1. Schematic diagram of the radial tracks
Fig. 2. Front view of a light-box
Fig. 3. Side view of one of the radial tracks

The light-boxes were 2 x 2 x 3½ in., and in each of them there was a small frosted 2-c.p. 115-volt lamp. Each box was mounted upon a block of hard-wood with metal flanges attached to the bottom of it to hold it snugly upon the track and yet permit it to be moved easily along the track. (See Fig. 2, for front view.) In the front end of each light-box was a 2 x 2 in. brass plate in the center of which was cut a broken circle whose dimensions were in accordance with Landolt's optotypes.[22] The diameter to the outer edges of the circle was ½ in.; the width of the circular band and of the break was ⅛ in. The circular center of the brass plate containing the broken circle was so constructed that the break in the circle could be rotated to any desired orientation; *e.g.* top, bottom, right side, upper left, etc. Immediately behind the brass plate was set a sheet of milk-white ground glass. When the lamp in the box was lighted, the stimulus-object was seen, in direct vision, as a uniform, pale-orange, incomplete circle, a plain C set in a certain orientation. We hoped that this object would give us a crude test of the *O*s' visual acuity for the various distances and eccentricities supplied by our apparatus. The light-boxes were moved along the tracks to the desired positions by means of a system of cords and balance-weights. A strong cord was attached to each end of the wooden block upon which the light-box was mounted, and then it passed over a system of pulleys. Fig. 3 shows the arrangement of cords, pulleys, and the counter-balance weights, *A* and *A'*, for one of the light-boxes. When weight *A* was pulled downward, the light-box moved forward; when weight *A* was raised, counter-weight *A'* pulled the light-box backward. The height of *A* for a given position of the optotype on its track was indicated on a vertical scale. The pulley mechanism fell without *O*'s field of regard as he observed in the darkened room.

The current used in the lamps was from a 110-volt direct-current line, and by passing it through a Nichols rheostat it was reduced to 50-volts at a current of 0.045 amp. Preliminary tests showed that a current of this energy, 2.25 watts, gave a stimulus-object of the most satisfactory intensity for our purposes. The physical intensity of the light for all positions at all eccentricities was kept constant, since it would have been inconvenient to vary it proportionately for the numerous settings of the stimulus-object used.

The fixation-point was a small point of light with an intensity just great enough to permit it to be seen as a clear, pale yellow point without irradiations. This point was set in the front of a light-box on the track extending straight ahead in front of *O* (Fig. 1). It was 1 m. from a point midway between the nodal points of *O*'s eyes. Both it and the centers of the broken circles in the 5 light-boxes at the left were in the horizontal visual plane. Gross head-movements which would interfere with constant fixation were eliminated by the use of a biting-board. A point of light was adjusted for each *O* to the right of the point of fixation so that it fell just within the right side of the blind-spot of his right eye. When the fixation was proper, this point of light could not be seen; but it appeared as soon as the fixation slipped to the left. Although this device furnished a check only of lateral slips of fixation to the left, we trusted that since the ob-

[22] See E. Landolt, Die Reform der Bestimmung der Sehschärfe, *Arch. f. Ophth.*, 64, 1906, 598-611.

jects to be observed were all in the left portion of the visual field it would reveal the slips of fixation which were most likely to occur.[23]

A Zimmermann kymograph controlled the 1-sec. exposure of the stimulus-objects. A sheet of paper with a horizontal slit upon the drum of the kymograph permitted a brush-contact to come against the revolving metal surface of the drum for 1 sec.[24] A system of switches enabled the experimenter to send the current from the timing device to any one of the light-boxes. The timing device, switches, and the balance-weights which the experimenter adjusted for the various positions of the light-boxes were all hidden from *O*'s view in a small booth made of beaverboard.

Procedure. The entire investigation was conducted in a dark-room. Ten minutes were allowed at the beginning of each experimental hour for partial dark-adaptation. The experimenter used in his booth a weak light while setting the light-boxes and recording results. This light was never on while the *O*s were observing and it did not interfere with their dark-adaptation since they kept their eyes closed while the light was on.

In the first series *O* made monocular observations with the right and left eyes as well as binocular observations in order to compare monocular with binocular vision under the conditions of our investigation. It was necessary to have such a comparison if we were reliably to determine how much each eye contributed to the binocular perception. The binocular and monocular observations were made in haphazard order, *O* being instructed before each observation to use both eyes, the right eye, or the left eye. When one eye was used, the other eye was covered with a blind. In the course of each experimental period (one hour) *O* made as many trials as time permitted.

The positions at which the stimulus-objects were presented were determined relative to the Müllerian horopteric circle.[25] They were either on the Müllerian circle or at the following distances before or behind it for the 5 eccentricities used: before, 20 cm., 40 cm., 60 cm. (excepting at eccentricities of 40° and 50°);[26] behind, 20 cm., 40 cm., 60 cm., 80 cm.[27] The stimulus-objects were presented at these positions in a haphazard order which was combined with the haphazard order of the binocular and monocular observations. In this series each

[23] When observations were made with the left eye alone, we had to depend entirely upon *O*'s unaided detection of slips of fixation since it was not possible to employ a similar point of light for the left eye which would not interfere with the light-boxes.

[24] The average of 50 trials was 1000.45 $\sigma \pm 70.12$.

[25] Although the horopteric circles of the individual *O*s varied slightly from one another we held the circle at a constant average value.

[26] The apparatus did not permit the employment of a position nearer than 40 cm. before the circle at eccentricities of 40° and 50°.

[27] For convenience the various positions will be referred to in this manner: I indicates the radial track 10° to the left of the median line of regard, II indicates the radial track 20° to the left of the median line of regard, etc.; "H" following the Roman numeral indicates that the stimulus-object was set at the position where the horopteric circle intersects that particular radius; *e.g.* 20+ following the Roman numeral indicates that the stimulus-object was 20 cm. *before* the horopteric circle; and 20— indicates that the stimulus-object was located 20 cm. *behind* the horopteric circle.

position was used only 3 times, once for the binocular observations and once each for the two sets (right, left) of monocular observations.

The following instructions were read to *O* at the beginning of each experimental period:

When the signal 'ready' is given, fixate the point of light directly in front of you. Make sure that the blind-spot light at the right is not visible. Shortly after the signal 'now' you will be shown a lighted area to the left of the fixation-point. Carefully observe this lighted area without change of fixation. When it has disappeared, close your eyes and report orally a descriptive account of what you have seen. Do not fail to report all slips of fixation which occur between the signal 'now' and the disappearance of the lighted area.

No mention of 'doubleness' was made in the instructions because we wished to see whether *O* would report it without an instructional hint. When a slip of fixation occurred, the observation was repeated later in the course of the experimental period.

Observers.[28] The eyes of all *O*s, excepting *D*,[29] were examined by an oculist, Dr. A. C. Durand. The results of the examination for the several *O*s were:

Fe: Vision without drops: R = 20/25; L = 20/40
with drops: R = 20/15 (lens: +cyl. 0.37, ax. 60) 9° exophoria
(cycloplegia) L = 20/40 (lens: +sph. 0.25, —cyl. 1.00, ax. 15) 1° left hyperphoria
Many fine dots in centers of crystalline lenses and many fine opacities in peripheries of both lenses. Abnormalities in both foveas, more marked in the left.

Fo: Vision without drops: R = 20/30+; L = 20/30
with drops: R = 20/30+ (lens: —sph. 0.25, —cyl. 0.37, ax. 100) 1° exophoria
L = 20/30 (lens: —sph. 0.25, —cyl. 0.75, ax. 75)
Media clear, except for remains of pupillary membrane across right pupil; fundi negative.

K: Vision without drops: R = 20/15; L = 20/15
with drops: R = 20/25? (lens: +sph. 0.50, + cyl. 0.75, ax. 105)
L = 20/30 (lens: +sph. 0.87, +cyl. 0.25, ax. 90) 1½° esophoria
Media clear; fundi negative.

L: Vision without drops: R = 20/20+; L = 20/15
with drops: R = 20/40-1 (lens: +sph. 0.50, +cyl. 0.75, ax. 105)
L = 20/50+2 (lens: +sph. 0.50, +cyl. 0.50, ax. 90) 2° esophoria
Media clear; fundi negative.

R: Vision without drops: R = 20/15?; L = 20/15?
with drops: R = 20/25? (lens: +sph. 0.25, +cyl. 0.25, ax. 90)
L = 20/30 (lens: +sph. 0.37, +cyl. 0.37, ax. 90) 1° exophoria
Media clear; fundi negative.

With all *O*s, vision was brought to 20/15 with lenses.

[28] The regular observers for the entire course of this series were Miss M. Davidson (*D*), Dr. S. Feldman (*Fe*), and Mr. C. W. Fox (*Fo*). Shortly before proceeding to Series 2, Mr. E. Kemp (*K*), Miss E. Lowenstein (*L*), and Mr. M. Roff (*R*) made a few observations in this series. *K* completed the first 75 observations of this series; *L*, the first 43; and *R*, the first 96. All *O*s were trained in psychological observation. *Fe*, *Fo*, *K*, and *R* were members of the staff. *D* and *L* were graduate students in psychology.

[29] *D* left the university before the ocular examinations were made.

Results. Before considering the few reports of "doubleness" (two objects) obtained in this series, we shall present the information gained from a careful examination of our *O*s' descriptive accounts concerning the binocular and monocular appearance of the broken circles at the various peripheral positions. In general, the results obtained from the 3 regular *O*s agree, and, although fewer in number, the results obtained from the other 3 closely agree with these.

With very few exceptions *O* perceived the object as a well-defined circle with a break in it which could be definitely localized only when the object was presented at an eccentricity of 10° either on the horopteric circle at 20— or at any of the + positions in front of the circle at that eccentricity. Even at these positions the break in the circle was not always correctly localized, but in most cases of incorrect localization *O* stated that he had seen the break clearly when the light appeared and then had forgotten its location when reporting.

At the positions either on the horopteric circle or in front of it on the 20° radius, as well as at 60+ on the 30° radius, *O* still saw the objects as orange or yellowish circles. At these positions, however, the contours were extremely blurred, and the break in the circle was usually not seen, although some of the reports showed that *O* had noticed a blurred area 'somewhere' in the circular band of light. At the other positions on the 10°, 20°, and 30° radii, the objects were usually seen as circular or oval masses of yellowish light in which slightly darker centers could sometimes be distinguished. At the most remote positions on these radii *O* often saw nothing more than blurred patches of light.

At the + positions on the 40° and 50° radii, *O* saw the objects as relatively large, irregular masses of whitish light in which brighter and less diffuse nuclei sometimes appeared. At the more remote positions, the objects were seen as relatively small, almost point-like, circular areas of diffuse whitish light.

All *O*s except *Fe* and *K* reported that when the light appeared it apparently moved forward and then receded as it disappeared. This phenomenon was so pronounced for *D* that she assumed that the experimenter was actually moving the light forward and then backward. The 4 *O*s who observed this phenomenon did not report it in all of their observations, but their reports do not reveal that it was favored either by monocular or binocular vision or by showing the light in some particular region of the visual field. *D, Fo,* and *R* also reported gamma-movement in a number of instances; *i.e.* the light seemed to expand when it appeared and to contract when it disappeared.[20]

It is interesting to note that the *O*s were often extremely inaccurate when they attempted to estimate the distance of the more remote lights. *Fe, L,* and *R* occasionally localized lights 60— and 80— on the 10° and 20° radii as being nearer than the fixation-point. A few less striking errors were also made.

A comparison of the binocular and monocular observations reveals no striking differences between them for any *O*. As a rule, but not invariably, the binocular perceptions were clearer and included more details of the figures than did the

[20] For the original description of this movement see F. Kenkel and K. Koffka, Beiträge zur Psychologie der Gestalt, *Zsch. f. Psychol.,* 67, 1913, 358-450.

monocular. A comparison of the monocular observations made with the right eye with those made with the left shows that for 3 *O*s the left eye gave slightly better perceptions than the right. *Fe* saw the object somewhat more distinctly with the left eye when the object was shown at the + positions along the 30° radius. At the positions along the 10° radius *L* saw the object more clearly with the left eye. *R* also saw the object more distinctly with the left eye when it was shown along the 10° and 20° radii. The reports of *D*, *Fo*, and *K* reveal no differences between observations made independently with the right and left eyes.

Turning now to the reports of doubleness we find that, except for one doubtful report made by *D*, *Fo* was the only regular *O* to report doubleness. When the object was presented at V 40+, *D* reported that, although she was not certain, it seemed that just before the exposure ended she saw two blurred lights linked together. In no other instance did *D* indicate doubling.

In 16 of the 38 binocular observations, *Fo* reported the object as double. In 10 of these instances *Fo* was certain that he had seen the object double; but in the other 6 instances he was slightly doubtful. These doubtful instances occurred when the object was presented at the more remote peripheral positions, where *Fo* found it difficult to decide whether he saw the blurred patch of light single or as two blurred patches which fused into one another. Since he favored the latter interpretation in these instances they have been included with the other reports of doubleness. Eight of these 16 instances of doubleness were for positions before the horopteric circle, and the other 8 were for positions behind it. Since only 4 of the instances of doubleness occurred when the stimulus-object was at 20+ or 20— positions, we may say that the farther the stimulus-object was removed from the horopteric circle the greater was the likelihood that the perceived object would be seen as double. The doubling was also more distinct in these instances, whereas when the horopteric circle was approached the object was often seen as two interlocking circles or as two overlapping areas of light. Five of the instances of doubling occurred when the stimulus-object was on the 30° radius; the other 11 instances were about equally distributed over the other four radii.

L was the only other *O* to report doubleness in this series. In 16 binocular trials she reported doubleness 5 times.[81] When the stimulus-object was presented at position I 60+ this *O* saw two clear broken circles standing side by side with a small intervening space; but in the other 4 instances she was not at all certain that she had seen the object double. Since *L* made only 16 binocular observations, it would be pointless to attempt to indicate from her results that for her doubling is more likely to occur at certain positions than at others.

Fo's detailed reports give us interesting information about the way in which the binocular doubling of the object occurred and about the criteria which he used to determine whether the more peripherally seen objects were single or double. Occasionally *Fo* saw the object immediately as two circles of light standing side by side; but more often he first saw one circle which then "split into two circles." These circles were either interlocked or they stood side by side. When the object appeared as a blurred area of light, *Fo* observed that often it expanded laterally and then separated into two blurred areas. Frequently this separation was not com-

[81] *L*'s reports of doubleness were for the following positions: I 60+, I 60—, I 20—, II 40+, and III 40—.

plete; but where *Fo* could distinguish in the blurred area two nuclei of more concentrated light, he reported that the object was double. Eventually as soon as *Fo* observed that the object or area of light was expanding he expected that it was about to become double. In several of his doubtful reports *Fo* stated that this expansion had occurred, but that he had been unable to decide with absolute certainty that it had resulted in doubleness. So far as we can determine from *L*'s reports she observed no such development of doubleness. Neither can we determine that she used in this series any other criterion of doublness than the appearance of two objects either standing side by side or interlocked.

Since these results can be best discussed in connection with the results of the other series, we shall postpone further treatment of them.

Series 2

Since the 2 *O*s who did report doubleness in the preceding series often had difficulty in deciding whether they saw the object as single or as two closely interlocked circles, or as two nearly coinciding areas of light, and since the other *O*s did not report doubleness when the object was set at positions where it seemed almost impossible that trained *O*s should not see double, we decided to perform another and similar series of experiments with a more satisfactory stimulus-object. After a few preliminary tests with objects of various shapes had shown that a vertical bar of light could be seen double more easily than could the broken circle, we substituted the vertical bar and repeated the procedure of the previous series.

Apparatus and procedure. Except for a few minor changes which are mentioned below, the apparatus was the same as that employed in Series 1. The same wattage was used in the lamps to illuminate the stimulus-object in the front of each light-box, and the same exposure-time and dark-adaptation were used.

A vertical slit whose dimensions were 1/16 × 1/2 in. was cut in the center of the brass plate in front of each light-box. This slit replaced the C. When the light was turned on, the object was seen in direct vision as a clean-cut narrow vertical bar of light.

Fixation was controlled by having *O* maintain the fixation-point directly in the center of a circular negative after-image.[32] This negative after-image was produced by fixating briefly a small circular band of light (outside diameter, 1/2 in.) of moderate intensity.[33] When the circle of light was turned off, the fixation-point

[32] This method of controlling fixation was proposed to us by Mr. Fox while he was observing in Series 1. In that series he once accidentally looked directly at the broken circle which was presented immediately to the left of his median line of regard, and when he regained his fixation he discovered that he had a circular negative after-image, but that the point slipped out of the center of the after-image if he slightly moved his eyes.

[33] Since slightly different intervals were required to arouse for the various *O*s a negative after-image which would last until the exposure of the stimulus-object had ended, the length of time that the circle of light was exposed was adjusted to meet the requirements of each *O*.

was presented immediately below. As long as constant fixation of the point was maintained, *O* saw the fixation-point in the center of the circular negative after-image, but the slightest wavering of fixation was immediately indicated by a movement of the fixation-point from the center of the after-image. The *O*s all found this method of controlling fixation much more satisfactory than that used in Series 1. Whereas they had always found it difficult to decide whether or not the light just within the blind-spot of the right eye had appeared while they were observing the object presented in the peripheral field at the left, they now found it relatively easy to detect any slips of fixation. Since we could not be certain that the circle of light presented to arouse the after-image did not disturb adaptation sufficiently to affect *O*'s peripheral perceptions, even though only a small area at the center of the retina was directly stimulated, we did not adopt this method of controlling fixation until we compared the results of two test-series made with all *O*s. In one test-series, the *O*s used this method of controlling fixation, and in the other they used the method employed in Series 1. Since we found absolutely no difference between the two sets of results, we assumed that we could safely use the second method.

Except for a slight change in the instructions, made necessary by the change in the method of controlling fixation, the procedure was the same as that followed in Series 1. The instructions were:

> When the signal 'ready' is given, place your teeth in the biting-board and fixate the circle of light directly in front of you. After a brief interval this circle of light will disappear and a point of light will appear directly below. Fixate this point of light so that it is seen exactly in the center of the negative after-image of the circle of light which you have just seen. Shortly after the signal 'now' you will be shown a lighted area to the left of the fixation-point. Carefully observe this lighted area without change of fixation and when it has disappeared close your eyes and report orally a descriptive account of what you have seen.
>
> If a clear-cut negative after-image of the circle of light fails to appear before the signal 'now' is given, report this fact immediately.
>
> Do not fail to report all slips of fixation which occur between the signal 'now' and the disappearance of the lighted area.

Results. *O*'s[34] descriptive accounts show that there were relatively few positions at which he saw the object as a vertical bar with sharp contours. There were many more positions at which he saw the object as a vertical bar with blurred or indefinite contours. At these positions there were often hazy streaks on each side of the object. At other times a halo of diffuse light surrounded the object. The following table shows for each *O* the positions at which he saw the object as having either clear or blurred contours.

It is important to note the wide range of positions at which most of the *O*s saw the object as a blurred vertical bar which frequently had vertical streaks of diffuse light on either side of it, since this appearance of the object probably influenced some of the reports of doubleness which are to be mentioned later.

[34] The *O*s, for the entire series were *Fe, Fo, K, L,* and *R,* all of whom observed in Series 1. *D* was no longer available.

O	Clear	Blurred
Fe	I, 60+	I, 40+ through 40− II, 60+ through 20− III, 60+ through 20+ IV, 40+, 20+ V, 40+
Fo	I, 60+ through 60− II, 60+	II, 40+, 20+ III, 60+ through 20+ IV, 40+, 20+ V, 40+, 20+
K	I, 60+, 40+ II, 60+	I, 20+ through 60− II, 40+ through 20− III, 60+ through 20+ IV, 40+, 20+ V, 40+, 20+
L	I, 60+	I, 40+ through 80− II, 60+ through 60− III, 60+ through 20− IV, 40+ through 60− V, 40+ through H
R	I, 60+ through H II, 60+, 40+ III, 60+	I, 20−, 40− II, 20+ through 20− III, 40+ IV, 40+ through H V, 40+ through H

All *O*s saw the object as a circular, elliptical or rectangular area of light at one or two of the positions on each radius immediately behind the more remote limit of the region in which a blurred bar was seen. At all the other positions the object was seen as a blurred patch of light or as a small, indefinite point of light.

As in Series 1, *Fo, L,* and *R* frequently reported that the light moved forward when it appeared and then receded when it disappeared. *Fo* also observed that often the light expanded laterally to the right and left when it appeared and then contracted again when it disappeared. In addition to this expansion and contraction of the object *Fo* observed upon a number of occasions a sudden and dynamic brightening of the whole visual field when the object appeared. He named this phenomenon phi-brightening because it was analogous to pure phi-movement and *apparently* was not dependent upon a diffusion of light from the object itself. Three excerpts from *Fo*'s reports may help to make clear what he meant by phi-brightening.

A very small and rather weak light which was localized rather far away appeared, and then was followed by a general brightening of the whole visual field. Then there was a darkening of the visual field and the light disappeared. . . . These changes were rather empty or analogous to pure phi-movement.

A rather dim orangish light appeared rather far away at the left. Then there was this very extended, sudden brightening of the whole visual field. . . . When I see the general brightening it isn't quite like that, but it is more a dynamic movement through that part of the field . . . and yet at the same time it has to do with brightening.

There was a welling, expanding irradiation of a forceful character which brought the light farther front and made it more impressive. Then there was a gradual recession. Here the brightening is definitely a diffusion around the light making it larger and more impressive, whereas the phi-brightening has nothing to do with any such diffusion.

This phi-brightening usually occurred when the object was presented in the more remote eccentric positions; but it was occasionally observed on the 10° and 20° radii for either binocular or monocular vision.

The perception was occasionally slightly more precise with binocular than with monocular vision when the object was presented at the positions where it was more likely to be seen as a blurred vertical bar than as a bar with sharp contours or as a rectangular or elliptical area of light. At some of these positions monocular observation with one eye was sometimes superior for a given *O* than with the other. The following table shows for each *O* the positions at which he saw the object more distinctly with one eye than with the other.

O	Right Eye	Left eye
Fe	IV, 40+, IV, 20+, V, 40+, V, 20+	II, 60+ through H III, 20+, 40+, 60+
Fo	All positions on I and II	III, 60+, IV, 40+, V, 40+
K	I, 60+ through H	III, 60+ through H
L	All positions on I	
R	All positions on III	

At all the other positions there were no discernible differences between the monocular observations right and left.

An examination of the *O*s' reports for evidence of binocular doubling of the object shows that two *O*s, *Fe* and *R*, never reported it with certainty. There were several instances, however, where they believed that the perceived object might have been double. When the object was presented to *Fe* at I 60+ he reported that he saw it as a well-defined vertical line with an uncertain suggestion of doubleness about it. At positions I 40+ and V 40+, *Fe* reported a vertical bar with a vertical dark streak down the center. In later series this *O* interpreted such an appearance to mean that the object was double. When the object was presented to *R* at V 20+ he reported that it was seen as a vertical orange light with some sort of duplicity about it.

The following table shows for *Fo*, *K* and *L* the positions at which they saw the object double.[85]

Fo	*K*
I, 60+, 40+, 20−, 40−, 60−, 80−	I, 40−, 60−, 80−
II, 60+, 40+, (40−), 60−	II, 20−, 40−, 60−, 80−
III, 60+, 40+, 20−, 60−	III, (20−), 60−, 80−
IV, 40+, 80−	IV, 20+, H, 20−, 40−, 60−, 80−
V, H (Total 17)	V, 20−, 60−, 80− (Total 19)

L

I, 60+, 40+, (H), (20−), (40−), (60−), 80−
II, (60+), (40+), 20+, (40−)
III, 40+, (20+), (H), 20−
IV, 40+, (20+), (20−)
V, (40+), (H), (20−) (Total 21)

[85] Parentheses around the symbol for a stimulus-position indicate that at that position *O* was not entirely certain that the object was double.

There are two instances of doubleness which *Fo* reported and several general tendencies shown by the results which should be mentioned. An examination of the above table shows that diplopia occurred slightly more frequently for *Fo* when the stimulus-object was placed behind the horopteric circle and that there was a decrease in the number of instances of diplopia with increased eccentricity. There was only one instance when *Fo* was uncertain whether the object had become double or had remained single. When the object was presented to him at position II 40—he observed that the vertical bar expanded to right and left, but he was not certain that it had become definitely double. In one instance, when the object was at V on the horopteric circle, *Fo* saw the object double when it was on the horopteric circle. At this position he first saw a single area of light, then there was a movement to the right, and finally he saw a second area of light to the right of the first.

K's results show that nearly all the instances of diplopia which he reported occurred when the stimulus-object was placed at positions behind the horopteric circle. There were also more instances of diplopia when the stimulus-object was on the 40° radius than when it was on any other radius. When the object was on the horopteric circle on the 40° radius *K* saw it as two spots of gray light.

Although *L* reported 21 instances in which she was at least moderately certain that the object was double, she was absolutely certain in only 7 of these instances. These 7 instances of certainty do not indicate that there was any given region in the peripheral visual field in which *L* was better able to distinguish between singleness and doubleness. There is evidence, however, that *the farther out* in the peripheral field the object was presented *the less likely she was to see it double.* It should be noted that 3 of *L*'s reports (less certain) of doubleness included observations made when the object was on the horopteric circle.

The ways in which doubleness appeared were practically the same for *Fo* in this as in the preceding series. Where binocular doubleness occurred, *Fo* usually saw the object first as single and then as double. There were two ways in which this change might occur. (1) Sometimes the object appeared to split into two which corresponded in size and shape to the initial single object. Sometimes there was complete separation of the two objects, but in other instances their adjacent sides overlapped. (2) In some instances the initial single object remained in its original position, but a second similar object slipped out from behind it and assumed a position to the right or left of the first. There were a few instances, however, in which, although *Fo* had not noted doubleness at the outset, he inferred that it had been there all the time since he had not *seen* the object single. The instances in which one object slipped from behind the other occurred only when the vertical bar was presented at positions within the horopteric circle. When doubleness occurred at positions beyond the circle there was usually a lateral expansion and then a separation into two objects. Although there were times when *Fo* could not distinguish sharply between the lateral expansion and a partial doubling of the object, he found that in general the doubleness was more clean-cut than it was in the preceding series with the broken circle.

The reports of *K* and *L* show no development of doubleness from an initial single object. If the object was seen double at all, it was seen double at the outset. These *Os* indicate, however, that they tended to use somewhat different cues as a basis for their reports of doubleness. *L* tended to see the more definitely formed ob-

jects double more frequently than the less definitely shaped objects, whereas *K* showed the opposite tendency.

Fo, *K*, and *L* also reported instances of monocular diplopia. Once when using the right eye alone *Fo* was certain that he saw the object break into two distinct objects. There were two other instances for the right eye and one for the left eye when the object appeared to him as if it might be double. *K* was certain, in one instance, that the object was double when it was looked at with the right eye alone, and there were four other instances for that eye when he 'believed' that the object was double. Using the left eye alone, *K* was very certain in 5 instances and moderately certain in three other instances that the object was double. Three of the instances of monocular diplopia occurred when the object was presented on the horopteric circle. The other instances occurred nearly as frequently when the object was presented within as when beyond the horopteric circle. The instances occurred more frequently when the object was presented on the 20°, 30°, or 40° radius than on either the 10° or 50° radii. *L* reported 22 instances of monocular diplopia, but she was absolutely certain in only 5 of these instances. Using the right eye alone 1 certain and 9 uncertain instances occurred, and using the left eye alone 4 certain and 8 uncertain instances. Approximately three-fourths of the 22 instances of monocular diplopia occurred when the object was presented at positions within the horopteric circle. They occurred most frequently on the 10° and 40° radii and least frequently on the 50° radius. Although the *O*s did not state explicitly in their descriptive accounts what the cue for monocular diplopia was, there are hints that the cue was a blurring or a diffusion of light on one or both sides of the object. It is significant that the positions at which monocular diplopia occurred were among the positions favorable to blurring or diffusion.

The results of this series will be discussed later in connection with the results of other series.

Series 2a

In this supplementary series *O* was instructed to draw as accurately as possible the appearance of the object when it was presented at selected positions for either monocular or binocular observation. As soon as the drawing was finished, *O* gave his descriptive account.

The series was undertaken for two reasons. We wished to obtain more accurate information about the single or double appearance of the object at various positions, and we hoped that the task of drawing the object would induce *O* to note carefully slight differences in the appearance of the object overlooked in the verbal description. The information gained from this series should also be useful in showing to what extent the various *O*s distinguished the less pronounced instances of doubleness and singleness.

The positions selected for this series were the horopteric circle, 40 cm. beyond, and 40 cm. within the circle, on each of the 5 radii. Employing a haphazard arrangement of the 15 positions used, the binocular and the monocular observations (right, left) were arranged in a haphazard order.

Except for the changes noted above, the *O*s, the apparatus, and the conditions were the same as in Series 2.

Results. The descriptive reports given by the *Os* in this series agree closely with those just described. Neither these reports nor the drawings show any striking differences, if we except the instances of diplopia, between monocular and binocular perception of the object at any given position.

Figs. 4-8 show typical drawings made by the 5 *Os.*[35a] First are given drawings made when the object was presented binocularly on the horopteric circle. All instances of binocular diplopia which occurred in this series are also included. Monocular results on the horopteric circle are omitted, since they are essentially the same as binocular. In order that the monocular observations may be compared, however, instances are included which show the object as presented to each eye alone at positions I 40—, II 40+, III 40— IV 40+, and V 40—. In general, the drawings from binocular observation at these positions are the same. Finally, all drawings which show monocular diplopia are included.

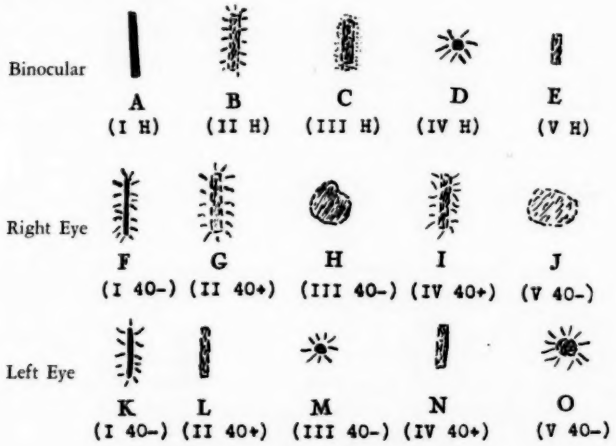


FIG. 4. TYPICAL DRAWINGS BY *Fe*

The *Os* stated from time to time that it was extremely difficult to represent properly in a drawing the exact appearance of the object. For this reason slight differences in the drawings should not be regarded as significant. *Fo,* especially, complained throughout the series that he could not represent in a drawing the dynamic changes which the object frequently underwent, such as movement forward and backward, expansion and contraction, or the sudden splitting of one object into two. In a few instances (*e.g.,* when the object was nothing more than a

[35a] In Figs. 5-8 all numerals not marked as minus should be read as plus values on the radii.

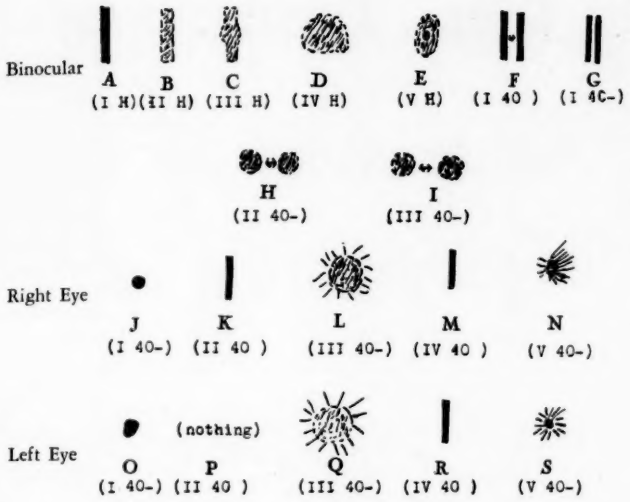


FIG. 5. TYPICAL DRAWINGS BY *Fo*

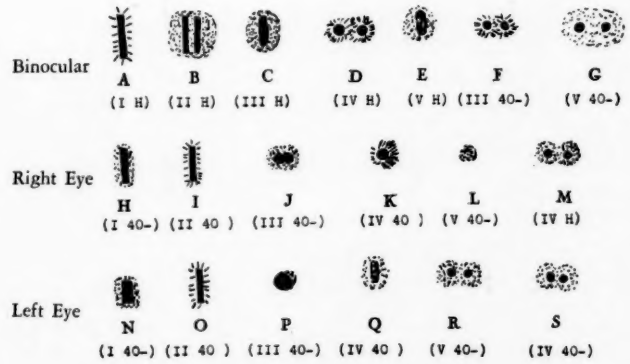


FIG. 6. TYPICAL DRAWINGS BY *K*

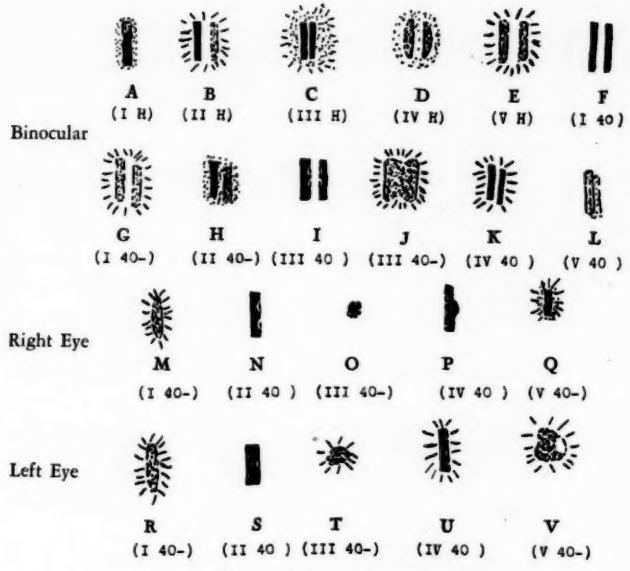


FIG. 7. TYPICAL DRAWINGS BY *L*

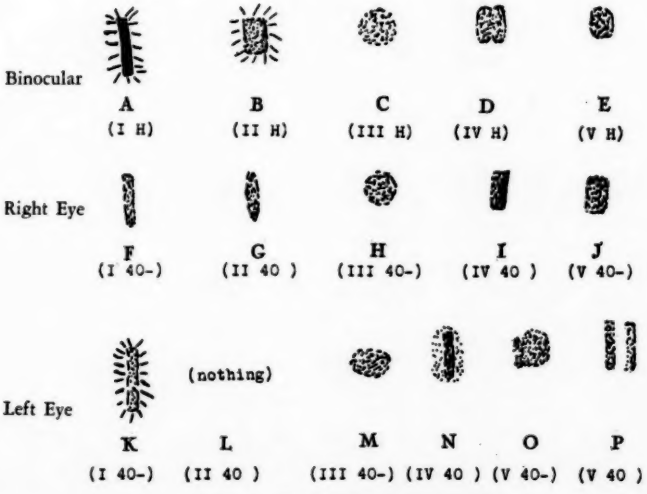


FIG. 8. TYPICAL DRAWINGS BY *R*

vague splash of light or when little more than phi-brightening occurred) he found it impossible to make any adequate drawing.

All *O*s adopted a relatively uniform way of representing various aspects of the object. Solid areas in the drawings indicate relatively solid and uniformly concentrated areas of light. Broken contours indicate that the boundaries of the object or of the diffuse light around it were hazy and indefinite, and ray-like lines indicate irradiations of light extending out from the object. Filmy, diffuse light, either within the limits of the object or beyond, is represented by stippling. The position of the object is given in parentheses below each drawing.

Fe and *R* did not report any instances of binocular diplopia, *Fo* and *K* each 4, and *L* 11. Drawings F to I, inclusive, Fig. 5; drawings B, D, F and G, Fig. 6, and drawings B to L inclusive, Fig. 7, represent the instances of binocular diplopia observed by *Fo, K,* and *L* respectively. *L's* drawing best show the various degrees of diplopia. For example, in drawings J and L two objects were not definitely separated while the *O*'s other drawings of diplopia show a pronounced separation. *L*'s drawings also indicate that there was no striking difference between the doubling on the horopteric circle and when the object was at a considerable distance from the circle.

Although *Fo* and *L* reported instances of monocular diplopia in Series 2, they did not report it at all in the present series. *K,* on the other hand, reported three instances of monocular diplopia, one for the right and two for the left eye (drawings M, R, and S, Fig. 6), and *R* reported one instance for the left eye (drawing P, Fig. 8).

When *K*'s diplopic drawings with monocular and with binocular vision are compared, no significant differences in the appearance of the double object are observable.[36]

Series 3

This series was undertaken in order to obtain a rough quantitative measure of the relative frequency with which doubleness occurs when the stimulus-object is placed at various positions either on the horopteric circle, within it, or beyond it. Since the results of our previous series were so varied in regard to diplopic vision under our conditions, we decided not to employ any refined quantitative method until a preliminary test had been made to show whether the results of such a procedure would indicate the region in the peripheral visual field in which objects most frequently appear single.

Apparatus and procedure. The apparatus, the stimulus-objects, the intensity of the light, and the method of controlling fixation were exactly the same as those employed in Series 2; but in this series all observations were made with binocular vision, and *O* reported only 'singleness' or 'doubleness' of the object without a detailed description.

[36] *Fo* and *R* made no drawings when the object was presented at the position II 40+ because at this position the stimulus impinged upon the blind-spots of their left eyes.

The stimulus-object was placed at each of the following positions on the five radii: on the horopteric circle; at 10+, 20+, 30+, and 40+; and at the corresponding — positions. This gave smaller differences between the adjacent positions than those employed in previous series, and also confined the investigation to a smaller region of the peripheral field. These changes permitted a more thorough investigation of the region in which the previous results had indicated that there was a change from single to double vision. The stimulus-object was presented 20 times in haphazard order at each of the 45 positions. Two successive presentations at any one position were avoided. Doubtful reports were permitted, but the same positions were repeated later in the period without *O*'s knowledge.[37]

Instructions. The following instructions were read to the *O*s at the beginning of each experimental hour.

When the signal 'ready' is given, place your teeth in the biting-board and fixate the circle of light directly in front of you. After a brief interval this circle of light will disappear and a point of light will appear directly below. Fixate this point of light so that it is seen exactly in the center of the negative after-image of the circle of light which you have just seen. Shortly after the signal 'now' you will be shown a lighted area to the left of the fixation-point. Carefully observe this area without change of fixation, and when it has disappeared close your eyes and report upon the singleness or doubleness of what you have seen. You may use the following terms: 'one,' 'two,' or 'doubtful.'

In making your judgments try carefully to avoid guessing or making inferences. Do not fail to report all slips of fixation which occur between the signal 'now' and the disappearance of the lighted area.

Results. Table I shows the frequency with which the *O*s saw the object double when the stimulus-object was placed twenty times at each of the positions.

TABLE I

NUMBER OF TIMES THE OBJECT WAS REPORTED AS 'TWO' AT THE VARIOUS STIMULUS-POSITIONS

O	Radius	Position of stimulus-object									Total
		40+	30+	20+	10+	H	10−	20−	30−	40−	
Fe	10°	18	16	18	18	20	20	19	20	20	169
	20°	19	14	18	20	19	20	19	19	19	167
	30°	18	17	19	19	20	20	20	20	20	173
	40°	10	13	17	15	16	17	18	18	16	140
	50°	8	9	16	16	19	18	18	19	20	143
Total		73	69	88	88	94	95	94	96	95	792
Fo	10°	17	20	19	17	15	15	18	18	18	157
	20°	11	16	19	18	13	14	17	18	19	145
	30°	17	19	18	15	14	14	9	16	16	138
	40°	5	11	13	12	8	9	6	5	8	77
	50°	2	3	2	1	0	2	1	1	1	13
Total		52	69	71	63	50	54	51	58	62	530

[37] The *O*s were the 5 who served in Series 2 and 2a. Although *Fe* and *R* had not previously reported binocular diplopia, they were included to determine whether they would observe it under the more specific instructions of this series.

TABLE I (*Continued*)

O	Radius	Position of stimulus-object									Total
		40+	30+	20+	10+	H	10−	20−	30−	40−	
	10°	0	1	0	2	1	1	8	18	17	48
	20°	7	3	5	13	12	17	18	19	19	113
K	30°	0	1	4	14	18	16	20	20	19	112
	40°	1	3	14	19	16	20	20	20	18	131
	50°	0	0	4	8	18	17	19	20	20	106
Total		8	8	27	56	65	71	85	97	93	510
	10°	20	19	19	20	19	19	19	19	16	170
	20°	20	20	20	15	9	5	4	5	5	103
L	30°	20	20	20	11	5	1	1	0	1	79
	40°	19	19	14	5	2	0	0	1	0	60
	50°	18	18	6	2	0	0	0	0	0	44
Total		97	96	79	53	35	25	24	25	22	456
	10°	2	0	0	0	0	0	0	0	0	2
	20°	0	1	0	0	0	0	0	0	0	1
R	30°	6	2	1	0	0	0	0	0	0	9
	40°	11	6	1	0	0	0	0	0	0	18
	50°	16	18	11	10	4	0	0	0	0	59
Total		35	27	13	10	4	0	0	0	0	89

There is a surprising amount of variability in the results of the 5 *O*s, as is shown by the differences in the total number of reports of doubleness and by the regions in the peripheral field where the doubleness occurred most frequently.

The table shows that for *Fo* and *L* an increase in the eccentricity at which the object was seen was accompanied by a decrease in the number of instances of doubleness. In the case of *Fo* this decrease was sudden after 30° and with *L* it was sudden after 20°. *Fe*'s results indicate a general increase in doubleness with increased eccentricity, but there were slight inversions at 30° and 50°. With *K* there was a general increase up to 40° and then a decrease at 50°. *R* showed an increase beginning at 30°, with most instances of doubleness at 50°.

If we consider the frequency of doubleness in relation to the nearness or remoteness of the object, we again find variability in the results of the 5 *O*s. Except for the inversion at 30+, *Fe*'s results show a slight increase in the instances of doubleness as we proceed from positions 40 cm. before the horopteric circle to the circle itself. From that distance on, the totals are approximately the same for all positions. A large amount of the increase occurred when the stimulus-object was presented on the 40° and 50° radii. In the case of *Fo*, there was an increase from 40+ to 20+, then a decrease, and finally an increase at 30— and 40—. These changes, as in the case of *Fe*, were most pronounced on the 40° and 50° radii. *K*'s results show a general increase with greater distance from the observer except for the inversion at 40—. These results show quite definitely that a ratio existed between the remoteness and vagueness of the object and the probability

that it would be seen double. *L*'s results in this respect are opposed to *K*'s. With *L* the probability of seeing the object double decreased with remoteness or with decreased definiteness. While there was very little variation along the 10° radius, along the other radii there was a sharp decline in the instances of doubleness from positions in front of the horopteric circle. *R*'s results show clearly that for him there was a very limited region in which doubleness might occur. The possibility increased with the approach to the positions nearest the *O* and with greater eccentricity.

When the object was on the Müllerian circle all of our *O*s except *R* reported a larger number of instances of diplopia than would have been expected in the light of the traditional doctrine. *Fe, Fo,* and *K* reported 50% or more of the instances on the horopteric circle as double.[38]

When we consider that *Fe* had not reported binocular diplopia prior to this series, it is worthy of note that in this series he reported many more instances of diplopia than any of the other *O*s. His reports of doubleness increased as the series progressed. Of his first 450 observations, he reported 359 as double, and in the second 450 observations he gave 433 reports of doubleness.

The results also show, as can be seen from Table II, that the *O*s varied greatly in the number of doubtful reports given. An examination of the table will show that when the stimulus-object was presented at positions on the 40° radius *Fe* and *Fo* had the greatest difficulty in deciding whether the object was single or double. Decision was unusually difficult at that eccentricity for *Fe*. *L* also had considerable difficulty when the stimulus-object was presented on the 40° radius, but her greatest difficulty was on the 20° radius. *K*'s reports of diplopia show that he tended to report ambiguous and ill-defined objects as double, and it is probable that any confusion as to singleness or doubleness immediately led him to report

TABLE II

NUMBER OF TIMES THE OBJECT WAS REPORTED AS DOUBTFUL AT THE VARIOUS STIMULUS-POSITIONS

O	Radius	Position of stimulus-object									Total
		40+	30+	20+	10+	H	10−	20−	30−	40−	
Fe	10°	4	10	5	6	2	3	0	0	1	31
	20°	6	2	5	5	2	3	3	2	3	31
	30°	9	9	7	1	2	1	3	3	2	37
	40°	5	9	31	14	11	19	11	5	7	112
	50°	13	17	11	8	9	3	9	9	7	86
Total		37	47	59	34	26	29	26	19	20	297
Fo	10°	0	0	1	3	3	2	5	3	0	17
	20°	1	2	3	3	7	5	8	3	5	37
	30°	6	2	3	7	12	8	9	7	4	58
	40°	1	9	11	3	10	19	14	6	12	85
	50°	1	4	1	1	4	4	2	7	4	28
Total		9	17	19	17	36	38	38	26	25	225

[38] It should be noted that, although in Series 2a *Fo* reported that he did not see the object when it was presented to the left eye at position II 40+, he reported in this series 11 instances out of 20 as being double at that position. Failure to see the object was probably due to the fact that at that position the object impinged upon the blind-spot of his left eye.

TABLE II (*Continued*)

O	Radius	Position of stimulus-object									Total
		40+	30+	20+	10+	H	10−	20−	30−	40−	
	10°	0	0	0	0	0	4	0	0	0	4
	20°	1	0	0	0	0	0	1	0	0	2
K	30°	0	1	2	0	0	0	0	0	0	3
	40°	0	0	0	0	0	0	0	0	0	0
	50°	0	1	0	0	1	0	0	0	0	2
Total		1	2	2	0	1	4	1	0	0	11
	10°	0	0	1	4	7	4	1	8	3	28
	20°	0	0	1	10	21	4	13	3	4	56
L	30°	0	0	1	14	5	4	1	0	4	29
	40°	1	5	13	6	7	2	3	1	0	38
	50°	1	4	9	7	3	1	1	1	0	27
Total		2	9	25	41	43	15	19	13	11	178
	10°	0	0	0	0	0	0	0	0	0	0
	20°	0	0	0	0	0	0	0	0	0	0
R	30°	0	1	0	0	0	0	0	0	0	1
	40°	4	2	0	0	0	0	0	0	0	6
	50°	2	1	3	5	3	0	2	0	0	16
Total		6	4	3	5	3	0	2	0	0	23

'double,' which would explain the scarcity of doubtful reports. It will be seen that *R*'s reports of 'doubtful' usually occurred when the stimulus-object was presented at positions where a report of diplopia was most frequently given.

Series 4

The results of Series 3 indicated the need for more exact information concerning the basis upon which the *O*s distinguished doubleness from singleness. The large number of doubtful reports given in the preceding series showed that the *O*s did not, in all instances, immediately note the difference. There were apparently a number of cues which served as a basis for the report. Some of these cues had already been brought out in the previous series; but it seemed possible that we might, since the observers were now more thoroughly familiar with the experimental situation, obtain more exact and complete information.

Apparatus and procedure. The apparatus and the experimental conditions were the same as in Series 3. The 5 *O*s, using binocular vision alone, were asked to describe the appearance of the vertical bar of light when it was presented at each of the 45 positions employed in Series 3. The instructions were practically the same as those used in Series 2.

O	Single	Double	Doubtful
Fe	3	35	7
Fo	24	6	15
K	25	20	—
L	30	5	10
R	42	3	—

Results. The preceding table shows for each *O* the number of instances of doubleness and singleness, and also the number of doubtful instances.[39]

Fe's reports of doubleness were distributed fairly uniformly over all 5 radii with a slight increase for positions beyond the horopteric circle. The majority of his doubtful reports occurred when the stimulus-object was presented within the horopteric circle on the 40° and 50° radii. *Fo*'s reports of doubleness occurred only when the stimulus-object was placed at positions on the 10°, 20°, or 30° radii, and were scattered for positions within or beyond the horopteric circle. The object was also reported double at II H. Most of *Fo*'s doubtful reports were given for positions beyond the horopteric circle and on the 40° and 50° radii. With one exception (II 30+), *K*'s reports of doubleness were given when the stimulus-object was placed between the most remote positions, 40— and 10+. More doubleness occurred when the stimulus-object was on the 30°, 40°, and 50° radii than when it was on the 10° and 20° radii. *L*'s 5 reports of doubleness were given when the stimulus-object was presented at positions I 40+, I 30+, II 40+, III 40+, and III 20+. Her doubtful reports show that she had greater difficulty in distinguishing between singleness and doubleness on the 10° radius and at near positions on the 40° and 50° radii. At all the far positions the object was seen as single. *R* saw the object single everywhere except at positions IV 30+, V 40+, and V 10+. No doubtful instances.

It will be seen that, although these results are much fewer in number, they agree in general tendency at least with those of the previous series. This permits us to assume that the *O*s' comments in this series may be regarded as applicable to the previous series.

The comments supply much information about the appearance of doubleness at various positions and about the cues used in deciding upon the singleness or doubleness of an object in various instances. *Fe*'s reports show that in those instances in which he saw the object double he perceived two adjacent areas of light separated by a black strip or by a vague black region. Most of *Fe*'s doubtful reports were given when he perceived a black strip but was unable to localize it definitely with reference to the rest of the object. In a few instances he reported that at first he saw the object as single with a dark streak down through the center, but that later he reapprehended it as two objects. The reports given for instances of doubleness which occurred when the stimulus-object was placed at the various positions on the horopteric circle indicate that there was no significant difference between the doubleness which occurred there and that which occurred when the stimulus-object was placed either 10 cm. beyond or 10 cm. within the horopteric circle. General comments which *Fe* made near the end of this series furnish information which enables us to understand why he never reported doubleness in Series 1 and 2, and reported it frequently in Series 3. Since he had never perceived doubleness in the peripheral field he did not look for it until it was mentioned in the instructions used in Series 3. As soon as he began to look for it he noted that the object which he had previously taken to be single was frequently separated into two by a central vertical black strip. Any indication of a black strip was then taken to mean that the object was double. He also reported that when there was

[39] Observations in which a doubtful report was given were not repeated in this series, as was done in Series 3.

any marked blurring of the light he tended to give a doubtful report, because he was not certain of the presence or absence of the black vertical strip or patch.

Fo's descriptive reports of doubleness and of the appearance of the object agree closely with the reports he gave in Series 2. As before, the object was almost always single when it first appeared, and then it split into two objects. The splitting of the object was usually intimately connected with gamma-movement, and in several instances *Fo* could not separate the two; *i.e.* the object could be regarded as a single object which expanded laterally or as one object which became double. Observations of this kind often led to doubtful reports. There were instances when *Fo* gave doubtful reports because he could not decide whether a vague, hazy patch of light at one side of the clearer area of light was actual diplopia or whether it was a fragmentary halo belonging to the brighter area. When the stimulus-object was placed very near on the 40° and 50° radii, *Fo* reported that the object had an unusual molten appearance which tended to prevent it from being seen double.

K's reports in this series give little information as to his criteria of doubleness when the separation was not definite. He stated that he tended to give reports of doubleness if the light was blurred and contained two regions of greater brightness, and also that, as the experiment progressed, he expected the distant objects to appear double.

Toward the close of the investigation *L* reported fewer instances of doubleness than at the beginning. She stated that at the outset she was unable to distinguish between singleness and doubleness when the object appeared at the more distant positions because she confused blurring and doubleness. Later she rarely reported that the object was double unless she noticed that there were two definite objects with a distinct separation. Nearly all relatively shapeless or blurred objects were reported as being single. Her doubtful reports usually indicated that the object looked as though it were about to separate, but that the separation was too slight for the object to be actually double. *L* also reported that she found difficulty in overcoming the tendency to anticipate doubleness in objects near her.

Since *R* gave so few reports of doubleness, we have very little information concerning the cues on which he based his reports. The 3 instances of diplopia occurring in this series were all instances where the object was definitely seen as two objects separated by a dark area. There is nothing in *R*'s reports which indicates why he occasionally saw the objects double when the stimulus-object was placed at the more advanced positions on the 30°, 40°, and 50° radii, and rarely saw it double when the stimulus-object was placed at similar positions on the 10° and 20° radii.

Discussion of Results

The results given above do not demonstrate the existence under the conditions of this investigation of an empirical horopter which corresponds in any way to the Müllerian horopteric circle. Neither the generalized results of all the *O*s nor the results of any single *O* indicate that objects lying in a region adjacent to the Müllerian circle will necessarily be seen single and that objects lying elsewhere will necessarily be seen double.[40] The

[40] Perhaps the results of *R* should be excepted, but they vary so widely from the results of the other observers that we shall disregard them temporarily.

most that our results do indicate in this respect is that, for a given *O*, there are regions in the peripheral field where an object will be seen single more frequently than it will be seen double. These regions varied greatly from one *O* to another, so that we must attempt to account for the enormous individual differences.

The results of Series 3 show best in quantitative form the variability of results. The explanation of this wide variability can be based most directly upon the descriptive reports and the comments which the *O*s made in the earlier series of this investigation. These reports show that often *O* could not tell immediately from the appearance of the object whether it was single or double. As the investigation progressed each *O* worked out for himself criteria of doubleness. Although specifically instructed not to make inferences, the several *O*s' criteria of doubleness differed so widely that the general results showed marked variability.

In peripheral vision an object undergoes changes in its appearance, and, at times, some of these changes were utilized by *O* to enable him to decide whether the object was single or double. There were instances when *O* actually saw two objects standing side by side with a definite separation between them, but at those positions where the object was decidedly blurred and vague *O* took certain characteristics of the object to mean that it was single or double.

Brief consideration of a few of the dioptric factors involved will enable us to see more clearly the basis for much of the difficulty that our *O*s encountered. One dioptric factor which may have given no little difficulty is that of monocular diplopia caused by monochromatic aberration of the lens.[41] We have evidence that it did play a part, since in those series where the *O*s made monocular observations they occasionally reported the objects double. It is possible that this factor gave rise to some of the reports of doubleness which were made when the stimulus-object was set on the horopteric circle.

Another dioptric factor which may have partially determined the results was oblique astigmatism. Ames and Proctor[42] have shown that in the peripheral field there is, with a given accommodation, a very limited field in which an object may be placed so that it is not seen blurred as a result of oblique astigmatism. Since few of the positions at which our stimulus-object was placed fell in this field, this factor alone did much to prevent the object from being seen as a clear-cut figure with definite boundary lines.

Although the reports indicate that the dioptric factors had a fairly uniform influence upon all *O*s, so far as the appearance of the object was concerned, there were significant differences in the way these changes were interpreted by the *O*s to enable them to decide whether the object was single or double.

[41] This phenomenon is discussed by Helmholtz, *Hdbh. der physiol. Optik*, I, (3d ed.), 1910, 161-163.

[42] A. Ames and C. A. Proctor, Dioptrics of the eye, *J. Opt. Soc. Amer.*, 5, 1921, 22-84, esp. 52-79.

One important factor in determining whether an object would be seen single or double was the instruction under which *O* worked. The importance of this factor is well shown in the case of *Fe*. During the early part of the investigation it did not occur to him that the peripherally seen objects might be seen double, and so he made no attempt to observe it. Later (Series 3), when *Fe* was instructed to report instances of singleness and doubleness, he reported doubleness more frequently than any other *O*.

To understand more adequately why *Fe* reported doubleness so frequently in Series 3 and 4, we must refer to his verbal reports. In the first few instances he noted that the two objects were separated from one another by a dark streak. He accepted the presence of a dark streak between areas of light as a criterion of doubleness. After a number of repetitions he then tended to report doubleness whenever he noted a black streak or area related to the object. Due probably to defects of his eyes,[43] *Fe* frequently perceived dark areas or streaks in the object. Once he was primed for these he naturally observed them more and more frequently as the experiment progressed.

Fo and *L* reduced the number of their reports of doubleness toward the end of the experiment, because with repetition they succeeded better in distinguishing diplopia from a figure composed of a vague object and a faint, detached halo. These two *O*s became more rigid in their criteria of doubleness, in that doubleness came to be something more seen than inferred. Having simplified and reduced their cues for doubleness, *Fo* and *L* reported less doubling when the stimulus-object was at the more eccentric and at the more remote positions.

Another factor which probably increased the number of *Fo*'s reports of doubleness was the connection which he observed between gamma-movement and separation. There were instances when *Fo* reported that he was uncertain whether the splitting had occurred; but there were probably other instances when he unintentionally inferred that the two had taken place together. In some of the instances of diplopia which he reported when the stimulus-object was on the horopteric circle he mentioned that gamma-movement had occurred.

K tended to report more doubleness with increased distance and eccentricity of the stimulus-object. When the object was seen as a blurred area he usually saw two regions of brighter light in it which he interpreted to mean that the object was double. On the other hand, a clear, definitely outlined figure was more insistently single.

There is nothing in *R*'s verbal reports which enables us to determine why he saw the object double much more rarely than the other *O*s and then only in a very limited region of the peripheral field. Evidently *R* did not report that an object was double unless he saw it as two objects with a distinct separation; but it is difficult to see why the object was seen thus only at the more eccentric near positions and not at the less eccentric positions.

Our results show that in this investigation seeing the object as single or double was relatively independent of its being imaged upon corresponding or non-corresponding retinal points. Central factors were usually much

[43] See the reference in the oculist's report above to opacities in the lenses and to abnormalities near the foveas.

more important in determining the singleness or doubleness of the object.

We may now attempt to explain why our results differ so radically from those of other investigators who have experimentally attacked the problem of the horopter.

One explanation which immediately offers itself is that others have generally used the criterion of the apparent plane; that is, finding the region of the visual field in which objects may be placed so that they appear to be in a straight line which is perpendicular to the median line of regard at the point of fixation; whereas we, in attempting to determine the empirical horopter, instructed our *O*s to report upon the singleness or doubleness of peripherally seen objects. From the point of view of the definition of the horopter as the locus of all points seen single with a given fixation of the eyes, our method of investigation seems psychologically more valid. Other methods, such as those used by Tschermak and his students, are based upon a more indirect, more physiological approach to the problem. By our method we are better able to discover the extent to which doubleness and singleness can be differentiated in various portions of the peripheral field.

Another factor which may help to account for the difference existing between our results and those of other investigators who have found that the empirical horopter is a band which surrounds and includes the Müllerian circle or lies within or beyond the Müllerian circle is that they worked with a lighted field while we worked with a darkened field. A lighted field permits more precise localization of the object, and knowledge of the location of the object with reference to the fixation-point might easily predispose sophisticated *O*s to see objects double only when they were much nearer or much more distant than the fixation-point was. We must not forget either that many of the investigators served as their own *O*s and thus, in many instances, knew fairly well the approximate location of the stimulus-object. It is not surprising, therefore, that, under their conditions, the empirical horopter was found in many instances to be a band. Since our *O*s worked under dark-adaptation they lacked all definite knowledge of the location of the stimulus-object. This statement is verified by their own comments. When, in their reports, they volunteered information about the location of the object it was shown that they frequently made gross errors. At other times, they stated that they were not at all certain how far away the object was. Therefore, since *accurate* localization played so small a part in their observations it is highly probable that at least some of the *O*s saw the object as double at certain positions, whereas they would have seen it single if it had been definitely localized for them as being, for

example, at about the same distance as the fixation-point. It is unfortunate that time did not permit us to complete another series of observations with the same *O*s working under light-adaptation in order to test the possible influence of relatively definite localization of the object upon seeing it double at various positions in the visual field.

There is still another way in which the employment of a lighted object under the conditions of dark-adaptation may have increased the number of instances of doublings. A non-fixated object standing out clearly from its surroundings is more likely to be seen double than is one which is less sharply differentiated from the background. The writer has frequently noted that in binocular peripheral vision bright clean-cut objects tend to become double while less sharply defined objects in the same position tend to remain single. If a high degree of differentiation from the background does enhance doubling, then the frequent appearance of doubleness for our *O*s becomes intelligible. It is possible that even *R*, who saw doubleness infrequently, might have seen it less frequently with weaker stimulus-objects.

We may say, as a concluding statement of this discussion, that there is no single group of factors which will adequately account for all the results of all our *O*s. It is obvious that the physiological factor of the stimulation of corresponding and non-corresponding retinal points was not nearly so important a determiner as we might have expected it to be. From our results we might almost conclude that corresponding points have little functional significance except at the fovea, so far as binocular single vision is concerned. The dioptric factors involved in peripheral vision alone will not adequately account for the results. A change in the appearance of the object due to dioptric factors was accompanied in the case of one *O* by an increase in the instances of doubleness, while the same change was accompanied in another *O* by a decrease in these instances. Apparently the only constant and direct determiner was that of instruction, formal, self, and occasional,[44] and since this determiner often took different directions in the various *O*s, the results varied widely and took sudden turns when new instructions of any kind replaced a previous instruction. The sudden appearance of frequent reports of doubleness in the case of *Fe*, when he was instructed to distinguish between the singleness and doubleness, is an excellent example of the effect of formal instruction. The fact that *L* instructed herself to report vague, unclear objects as single, while *K* instructed himself to report them as double is an example of self-instruction tak-

[44] M. Bentley, *The New Field of Psychology, Pt. i, The Psychological Functions*, 1933, 78-79.

ing opposite directions. One of the many instances of the effect of occasional instruction in determining the results obtained in this investigation was *Fo*'s tendency to report doubleness when he observed gamma-movement. He did not instruct himself to report doubleness when gamma-movement occurred, but after doubleness and gamma-movement had occurred together in a few instances he found that the appearance of the latter phenomenon alone frequently primed him to report doubleness even when he was not certain that he had seen a double object.[45]

SUMMARY

This study was undertaken to investigate by experimental means the significance of the horopter in visual perception. An attempt was made to determine under dark-adaptation those regions in the left portion of the visual field where a lighted object on a lightless ground is seen single and the regions where it is seen double. (1) Preliminary trials were first made with a broken circle as stimulus-object. The *O*s were instructed to report a full description of the object both in monocular and in binocular vision. Diplopia was not mentioned in the instructions because we wished to see whether *O* would discover it for himself. (2) When a vertical bar had been substituted as a stimulus-object—in order to facilitate the discrimination of single and plural objects—a like set of binocular and monocular observations was carried through. (3) In a supplementary series the *O*s made drawings of the vertical bar as seen with one or both eyes at certain selected positions. (4) Then each *O* made a series of 900 binocular observations in which he reported the object as single or double when the stimulus-object was presented at any one of the 45 positions (9 distances along each of 5 radii). (5) Following this series the *O*s were again instructed to describe the object and to state the criteria for doubleness at each of the 45 positions.

We found that a group of central factors is a more important determiner of singleness and doubleness than are retinal correspondence and non-correspondence. Since these central determiners varied in kind and in precision among our various *O*s, notable individual differences came to

[45] Since the completion of the experiments reported here, we have begun a new series of experiments which indicate that a shift from binocular to monocular, or from monocular to binocular, apprehension of objects in the peripheral visual field enables the *O*s to distinguish more accurately between singleness and doubleness. The shift from binocular to monocular vision, or the reverse, is accomplished by covering either the right or left eye by silent blinds for the first or second half of the exposure-period. The complete results will be published later, but we may mention here that they also fail to confirm the geometrical horopter.

light in their binocular perception of single and plural objects from a single eccentric source of illumination. The results obtained by this experimental procedure fail to demonstrate any fixed region corresponding in any way to the Müllerian circle wherein objects are seen as single and outside of which they appear double. Since the doctrine of corresponding points implies the existence of a region in the binocular field wherein objects will be predominantly double for all observers with normal vision, and, since such a region or *horopter* was not found in this study, the significance of the doctrine of corresponding points is called into question, so far as it applies to the peripheral field under the conditions of this investigation.

PROGRESSIVE CHANGES IN NON-FOVEAL PERCEPTION OF LINE PATTERNS

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It is a commonplace that the activities of any living animal can be fully understood only when the developmental history of the animal has been made out. The commonplace applies as well to psychological as to biological history, for the psychological functions of the individual are no more static and fixed than the biological functions are. They are constantly modified under use and growth. Just as the body grows and develops, so does every psychological function undergo significant changes which may continue either for a short time or throughout a large part of the life-course of the individual.

The present study is concerned with one of these psychological operations, *i.e.* with perceptive apprehension, and with the changes which this operation undergoes during an exceedingly small part of the entire history of the individual. If we succeed in producing, under experimental conditions, significant functional changes of a progressive sort, we may be able to use these changes as an indicator of those larger and slower modifications which are ordinarily spoken of as 'individual development.' The primary necessity of our experiments has been to induce a functional performance at a crude and elementary level and then to watch it 'develop' at a relatively rapid rate. To meet this necessity we have chosen certain visual perceptions of unconventional pattern which depend for their initiation upon the excitation of limited, extra-foveal regions of the retina.

After the earlier post-natal years our perceptive functions are generally so definite in their courses that it is not easy to determine the developmental changes in them. Progressive modifications in perception, therefore, must be set up under conditions which permit them to be inspected, *i.e.* perceptions must be experimentally initiated which lack the stable and specific character of our definite and fixed operations and a means devised for carrying them through a series of modifications. The means adopted in the present experiments have been (1) to use stimulus-patterns that are relatively unfamiliar to the observer, and (2) to present these patterns under conditions which interfere with their full effectiveness. Whereas the

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device commonly employed in previous studies to bring about such changes has been the presentation of a stimulus-object under conditions which are *increasingly favorable to its effectiveness* (*e.g.* by extending the time of exposition), in the present investigation *constant conditions of presentation* have been used, and bare exercise under precise instruction has been depended upon for functional modification. The purpose of this method has been to avoid serious difficulties which attend the interpretation of those changes which are directly attributable to a shifting stimulus. The visual modality of perception we have used mainly because the imperfections and the limitations of peripheral vision provide a feasible and useful means for initiating perceptions of a relatively primitive sort. Possibilities are thus provided for determining whether progressive changes can be brought about, and, if so, for determining the nature of these changes.

History of the Problem

General theories of the development of behavior. In the recent literature upon modifications in organic activity, there seems to be a continually recurring controversy between those who conceive of functional development as proceeding from original unitary totals toward organizations of greater and greater differentiation and those who conceive of it as proceeding in the opposite direction; namely, of increasing unification of originally independent parts.

In an account of recent progress in comparative psychology, Kafka suggests that both directions of change are conceivable.[1] Theories of the development of behavior he believes, therefore, should not be restricted to either type alone. He cites experimental evidence to show that there are two basic types of behavior corresponding to the motility of the organism. In the case of partially non-motile organisms (organisms that move with difficulty), the basis of behavior consists in local reactions which only gradually embrace more distant bodily parts and lead finally to reactions of the whole body. In the case of highly-motile organisms, on the other hand, the original reactions consist of those movements of the entire body by means of which the organism changes its place in the environment and only later in local reactions without progressive locomotion. Kafka believes that if this rule should be found to be generally valid, it would mean that the total coördination of behavior comes about in two ways; in the case of the less motile organisms by integration, *i.e.* by the increasing unification of relatively isolated reactions, and in the case of the more motile organisms by the differentiation of original unitary total reactions into relatively independent individual reactions. The assumption of the unitariness of behavior alone as an explanatory principle is, as Kafka believes, not justified,—for

[1] G. Kafka, Die Bedeutung des Behaviorismus für die vergleichende Psychologie und Biologie, *Ber. XII. Kongr. für der deutschen Gesellschaft für Psychol.* (Hamburg, 12.-16. April, 1931), 1931, 213-255.

in certain cases the unitariness itself requires explanation. If, for example, the unitariness is to be considered in the true sense of the word as an integration and not as a bare summation, then, in the single reactions which apparently originate independently, a regulatory principle must already be effective which implies a reciprocal dependency of the individual reactions upon each other. It is seen therefore, says Kafka, "dass weder ein Nativismus, der von stationären Gestalten ausgeht, ohne sich um ihre Entstehung zu bekümmern, noch ein Empirismus, der voneinander unabhängige Elemente auf dem Weg einer bloss additiven Assoziation verknüpfen will, sondern erst eine genetische Methode den Tatsachen gerecht wird, welche zwar eine ursprüngliche funktionelle Beziehung zwischen allen Elementen des Seelenlebens voraussetzt, aber sowohl die Integration wie Desintegration oder Differenzierung sekundärer aus relativ primären Strukturen zum Gegenstande der Untersuchung macht."[2]

Sander,[3] too, in a comprehensive survey of the results of Gestalt psychology, comes to the conclusion that *Gestalten* may change in two such ways. He declares that "Gestalten . . . stehen zwischen zwei polaren Gegensätzen ungegliederter Ganzheitlichkeit und der Stückhaftigkeit zusammenhangloser Teile. Sie stehen in dieser Mitte als etwas von beiden Seiten gleichsam Erstrebtes. Die Seele ist angelegt, diffus, Ganzheitliches zu durchformen und umgekehrt unverbunden Stückhaftes als Glieder zu einem gestalthaften Ganzen zusammenzuschliessen."[4] Articulated or differentiated *Gestalten,* that is to say, come into being either from the direction of unitary totals or from that of a coexistence of independent parts. He cites experimental evidence which will be examined later.

Two other relevant writers upon development are Coghill and Thorndike. Since the directions of development reported by them find representation also in the schemas of Kafka and Sander, it is not necessary to describe their doctrines in detail. Coghill's[5] investigations show that, in Amblystoma, development begins with certain generalized movements of the total organism and proceeds by a gradual and continuous differentiation of localized movements in various members. Thorndike[6] would have development of behavior proceed by the formation of 'connections' between 'situations' and 'responses.' His conception of bodily functions in development rests upon a fairly naïve theory of the integration of simple and independent neural units.

Conditions of visual form-perception. The investigations of conditions of form-perception fall into three classes: (*1*) those experiments which primarily deal with the central conditions of perceiving; (2) those primarily concerned with the periph-

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[2] *Ibid.,* 223.

[3] F. Sander, Experimentelle Ergebnisse der Gestaltpsychologie, *Ber. X. Kongr. für Exper. Psychol.* (Bonn, 20.-23. April, 1927), 1927, 23-88.

[4] *Ibid.,* 61.

[5] G. E. Coghill, *Anatomy and the Problem of Behavior,* 1929. Coghill attributes this behavioral development to the nature of development in the nervous system, *i.e.* the increasing differentiation in behavior he assumes to be the counterpart of neural growth. Kuo has recently modified this correlation on the basis of experiments upon bird-embryos. He thinks that Coghill has under-rated the importance of environmental conditions to the behavior of the organism. (Z. Y. Kuo, Ontogeny of embryonic behavior in aves: III. The structural and environmental factors in embryonic behavior, *J. Comp. Psychol.,* 13, 1932, 245-271.)

[6] E. L. Thorndike, *Educational Psychology,* I, 1913; *Human Learning,* 1931.

eral conditions; and (3) those which aim to discover the direct effects of exercise of the perceptive functions with maximal constancy in both sets of conditions.

(*1*) *Central conditions.* Studies are here included which aim to investigate the characteristics of such functions as perceiving and remembering when the influence of the stimulus upon these functions is limited. Sander states that such studies contribute to our understanding of the way *Gestalten* come to arise.[7] If his contention is correct, then these studies are relevant to the present investigation. The experiments are of two kinds: (*a*) those which have investigated the activity of perceiving when the influence of the stimulus is reduced to a minimum; and (*b*) those which have investigated the changes in reproduction of perception at successive intervals of time after the original perception.

(*a*) *Experiments with 'reduction' of influence of stimulus.* In his investigation of entoptic phenomena Purkinje found that, when the sun is viewed through the spread fingers moving back and forth before the closed eyes, the light-shadow patterns which arise show regular and symmetrical forms.[8] Goldschmidt later confirmed these observations and found similar characteristics in entoptic phenomena produced in other ways,[9] *e.g.* by pressure upon the closed eyes.

Another group of experiments cited by Sander as throwing light upon the dispositional tendencies of the organism are those of Rüsche[10] and Kirschmann.[11] Rüsche investigated the visual forms outlined by successively presented points briefly exposed, and concluded that there was a rounding out of contours. Kirschmann investigated the recognition of geometrical figures in indirect vision. He, too, found a tendency to round out contours. Polygons, for example, were seen as circles. There seems to be a similarity in the results of these various experiments in showing a tendency toward simpler and more regular figures when the relative effectiveness of the stimulus is decreased.

(*b*) *'Reproduction' experiments.* Here the stimulus is used merely to initiate the function. The *O*s are given a perceptive experience and then instructed at ever-increasing intervals to describe or to reproduce by drawing their memory of the original perceptual object.

In 1897, Philippe investigated changes undergone by memory-images.[12] The studies were first carried out with 6 adult *O*s. Five objects were employed, three of which were common and the other two less common (a screw with a flat head, a tailor's button, a tie pin with a cameo of the head of Caesar, a fleur-de-lis stamped in copper for hanging a picture, and a small Japanese mask of lacquered plaster). Having closed his eyes, *O* was presented with the object on the palm of his right hand and was instructed not to move his hand. He then drew a picture of the perceived object. Another drawing was made of the same object after *O* had handled it. He was then shown the object, which ordinarily differed little from his last presentation. At the end of 15 or 20 days, *O*, who had considered the experiment closed, was asked for new drawings. The same request was made a month later and thereafter at intervals of one or two months. The experiments were repeated on 30 pupils (5-8 yr. old) of the nursery schools of Paris. The changes in the "mental images" observed took three directions; (1) the image disappeared through

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[7] F. Sander, *op. cit.*, 57.

[8] J. Purkinje, *Beiträge zur Kenntnis des Sehens in subjektiver Hinsicht,* 1819.

[9] R. H. Goldschmidt, Beobachtungen über exemplarische subjektive optische Phänomene, *Zsch. f. Psychol.,* 76, 1916, 289-436.

[10] F. Rüsche, Über die Einordnung neuer Eindrücke in eine Vorhergegebene Gesamtvorstellung, *Zsch. f. Psychol.,* 10, 1900, 265-338.

[11] A. Kirschmann, Über die Erkennbarkeit geometrischer Figuren und Schriftzeichen im indirekten Sehen, *Arch. f. d. ges. Psychol.* 13, 1908, 352-388.

[12] J. Philippe, Sur les transformations de nos images mentales, *Rev. Philos.,* 43, 1897, 481-493.

a weakening or a confusion of details, (2) the details of the image were replaced by others which changed it to a different form, and (3) the images evolved through a series of changes toward an image-type, representing the groups to which the original image seemed to belong.

Similar directions of change were later found by Wulf,[13] who presented to his *O*s a given visual form for 5-10 sec. (depending upon the complexity of the form) and determined the changes occurring in the sequence of reproductions of these drawings through a period of time. Wulf concludes that the changes produced are of two kinds, sharpening (Präzisierung) and levelling (Nivellierung). Sharpening is exaggeration or emphasis of a characteristic or peculiarity of the presented figure. Levelling is omission, toning down, or weakening of a characteristic. Wulf distinguishes three specific kinds of changes that may cause sharpening or levelling: (1) normalising (*Normalisierung*), *i.e.* a change in the direction of a familiar object, (2) emphasizing (*Pointierung*), *i.e.* exaggeration of a characteristic or peculiarity, and (3) structurally-conditioned change or direction of change (*Struktiv Veränderung*) which is determined independently of (1) or (2)—*e.g.* a form tends to be symmetrical even where a normalising apprehension works in the direction of asymmetry. *Gestalt* psychologists cite such experiments as Wulf's as furnishing further support of the law of *Prägnanz, i.e. Gestalten* tend to change in the direction of greatest simplicity and regularity, increasing the prominence of the essential features of the *Gestalt*.

In order to check the conclusions of Wulf, Gibson[14] used two series of fairly simple geometrical forms, one series made up of straight lines and one made up of curves. The first group of 14 figures was visually exposed for $1\frac{1}{2}$ sec. each and the *O* reproduced what he could. Then there was a repetition of the exposure and another reproduction. In some cases a third exposure was given. With a second group of figures the exposure-time was 2 sec. The author named five tendencies which were apparent in his results. (1) Object-assimilation, "the term used to describe the phenomenon in which the perception of the figure involves visual or verbal imagery of some familiar object or shape, and where the reproduction which follows is clearly changed so that it more nearly resembles the familiar thing than does the stimulus-figure." (2) Verbal analysis, which "occurs when the stimulus form, instead of being associated with a single familiar object or shape, is analysed verbally in any of a variety of ways and when the reproduction is so changed that it is wholly or partially a product of the verbal analysis rather than a representation of the form itself." (3) Figure assimilation, which "in its simplest form may be defined as the phenomenon where one of the stimulus-figures is apprehended by the *O* as in some respect similar to a second stimulus-figure, and where the ensuing reproduction is clearly changed so as to resemble the second figure more closely than does the original." (4) Completion and disintegration, and (5) Rectilinearity. Gibson criticizes Wulf on the ground that the figures used by Wulf *could* change in only two directions and also because his own results seemed to show that it is the perceptual habits rather than the laws of configuration (*e.g.* Prägnanz) which condition the changes observed. "The changes observed in these experiments," says Gibson, "have not been interpreted as evidence for a single law determining the changes of configurations, but rather as evidence for the existence of perceptual habits which have arisen in the individual during experience. In general, the nature of a change found in the reproduction depends upon the manner in which the figure was apprehended."[15]

(2) *Peripheral conditions.* Since the present study involves the presentation of patterns in peripheral vision, it is relevant to consider those investigations which

[13] F. Wulf, Beiträge zur Psychologie der Gestalt; VI. Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psychol. Forsch.*, 1, 1922, 333-373.

[14] J. J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.*, 12, 1929, 1-39.

[15] *Ibid.*, 39.

deal with the effect upon perception of the special physiological and anatomical conditions of the peripheral retina and of the dioptric media.

Ames and Proctor undertook to determine the characteristics of the images of those objects upon which the eye is not focussed.[16] Their studies give a detailed account of dioptrics of the peripheral retina. The authors conclude that

"the retinal picture . . . can be described in general terms as being a picture in which objects at the center of interest, or focus-point, are depicted in considerable detail but not with microscopic detail. . . . Objects lying to one side of the line of vision are . . . less clearly depicted, the lack of clearness increasing with the angle of obliquity, the accentuation of detail and edges in such objects depending upon their position relative to the center of interest. . . . All oblique objects are distorted and changed in shape varying with the amount of their obliquity. This distortion is shown in the bowing out in their central portions of straight lines which do not pass through the center of interest and a reduction in size of oblique objects."

The authors have carefully mapped this "barrel" distortion. The results of their studies need not here be considered in further detail since the mechanism of peripheral vision is not the primary concern of the present investigation.

Zigler reviews previous investigations upon perception in peripheral vision and proceeds to a comprehensive investigation of the subject.[17] Zigler's object was "to determine whether qualitative differences in the perception of form can be found at various places in the peripheral field of vision." He argues that previous studies of the nature of form in peripheral vision (Aubert, Kleitman and Blier, and Geissler) do not provide an adequate solution to this problem. Zigler's method was gradually to move a given form from the periphery to the fovea, instructing the *O* to report changes in perceptive experience. From his results he concludes that "four different fields of apprehension of form are demonstrated in the visual field in the four major quarters." In the outermost (figureless field) the background only is perceived; there is no figure. In the next field (formless figure) an unorganized mass of meaningless marks or lines is vaguely seen and the principal dimension of the figure reported. In the third one (form-like figure) there is an uncertain figure-like formation which suggests several names in succession. Mutilated figures and the alteration of details are not perceived. In the innermost region (clear figure) details are accurately perceived. Zigler believes that his results support the assumption that differences in form-perception are due to a retinal factor. He decides that "the reduction in density of retinal terminals in the periphery and the fact that an optic fiber in indirect vision supplies a number of terminals, whereas in the fovea each optic fiber supplies only one or at most two terminals, is probably a sufficient explanation of the difference existing between central and peripheral perceptual adequacy." Zigler grants that "central or cerebral factors play a part, since in the form-like region a tentative classification tends to produce alterations in the appearance of a vaguely perceived figure." But he concludes that "the prepotency of retinal function is indicated by the fact that the several forms tentatively assigned in this region are invariably reduced to the true form as soon as the figure is clearly seen."

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[16] A. J. Ames, C. A. Proctor, and B. Ames, Vision and the techniques of art, *Proc. Amer. Acad. Arts & Sci.*, 58, 1923, 36.

[17] M. J. Zigler, B. Cook, D. Miller, and L. Wemple, The perception of form in peripheral vision, this JOURNAL, 42, 1930, 246-259.

Galli has investigated the perception of forms in peripheral vision,[18] using the same method as that used by Zigler. *O* was shown, indirectly along four meridians, simple geometrical figures—letters, numerals, and "meaningless figures." Galli finds five phases in the perception of figures under his conditions: (1) a vague impression of an indefinite thing in the field of vision, (2) the substantiation of the presence of an indefinite object, (3) the appearance of rudimentary forms or figures, similar to known figures, (4) perception of form, and (5) designation.

In this method (Zigler, Galli, and others) of employing conventional forms, figures, and symbols, it is difficult, if not impossible, to estimate the effects of recognizing, instead of 'seeing,' the known forms. To touch off a verbal name or other epithet, it is only necessary that the stimulus-pattern should be sufficient to suggest 'that form.' Practice and repetition make *O* more and more expert in distinguishing the several members of the series (seen at times with full foveal adequacy). The experiment comes to be, therefore, more a process of coaching in the interpretation of indistinct signs than a discovery of retinal adequacy at various removes from the fovea.

(3) *Influence of functional exercise upon perception.* Gottschaldt found that certain simple 'configurations' were not recognized by his *O*s when they became geometrical parts of a larger configuration, which tended to mask or destroy them as simpler figures.[19] He found, too, that, even though the simpler figure had been previously presented to an *O* as many as 500 times and had become completely familiar, this masking effect of the total persisted. Despite the many repetitions of the smaller figure it was not recognized when 'embedded' in the larger. The simpler figure was not observed at all when the *O* was instructed merely to report everything seen, and it was observed only rarely when *O* was specifically instructed to look for it. This result has been regarded as destroying the theory that organization, present in phenomenological experience, can be explained by past experience. Gottschaldt contends that "hier hatte sich ergeben, dass die Wirkung solcher gehäuften Wiederholungen auf das Wahrnehmen von Figurationen an sich überhaupt äusserst gering, bei den dort verwendeten Graden von Gestaltbildung nahezu gleich Null zu setzen ist."[20]

Dickinson's problem was to obtain an "introspective description" of the temporal course of perceptive experience.[21] He briefly presented simple "meaningful forms" (arrangements of letters and playing cards). On the basis of reports of his *O*s, he concludes that "there appear to be three levels in the progress of . . . visual perception from its initial moment to its termination." These levels he designates as visual pattern, generic object, and specific object. By 'experience as visual pattern' Dickinson means "that experience here is a mere 'thereness,' clear in contour and in detail of illumination, but lacking in logical meaning." By 'experience as generic object' he means that these forms have taken on generic meaning. By 'experience as specific object' he means that "a definite label has accrued and the forms have

[18] A. Galli, La percezione della forma nella visione periferica, *Pubbl. Univ. Cattol. d. S. Cuore*, 6, 1931, 1-27.

[19] K. Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren, *Psychol. Forsch.*, 8, 1926, 261-318; 12, 1929, 1-37.

[20] *Ibid.*, 12, 1929, 2.

[21] C. A. Dickinson, Experience and visual perception, this JOURNAL, 37, 1926, 330-344.

taken on a stability which previously they lacked." This study of Dickinson's is concerned, not with changes taking place in the perceptive function as a result of repeated exercise, but rather with the temporal course in a given perceptive performance.

Dickinson seems not to have been aware of a similar, but more analytical, description of perceptive levels or stages carried through several years before by Rogers.[22] Rogers distinguishes figurational ('nonsense' objects), depictive (specific objects), abstractive (presentation of a class), and symbolic perceptions.

A still earlier experiment upon progressive modifications in perception was that of Judd and Cowling.[23] These authors exposed for 10 sec. a figure made up of joined straight and curved lines. *O* was required to draw the perceived figure. After another exposure of 10 sec., *O* made another drawing. This continued for ten or more times with the same figure. The drawings were made under various conditions, with closed eyes, with seen movements of the hand, with both movements and resulting drawing seen. The authors conclude that "the recognition of the figure is gradual, some *O*s beginning at the beginning of the figure and working it out in detail, others beginning at other points in the figure; that the greatest amount of error is in the middle of the figure; that there is a difference in the rate of mastery of the size and relative position of the lines; and that in certain cases parts of the figure may improve for a time and the same parts may later be partially or completely overlooked." The emphasis in the experiment is upon improvements in accuracy, a matter obviously different from the nature of perception itself.

Hempstead investigated the perception of gray forms slightly different in brightness from their backgrounds.[24] The figure was exposed for 5 sec., and the *O*s then drew and described it. The figure was exposed again, with an increase in the brightness between the figure and the ground, and another drawing was made. There were 5-30 changes in brightness and 5-30 drawings made for each of 142 figures. Hempstead concluded that figures under these conditions develop according to two principles, symmetry and similarity, and that "each observer has certain habits of illusion, or certain typical modes of associative completion, which persist with certain modifications throughout his record."

Wohlfahrt studied the genesis of *Gestalten* in the "developed consciousness."[25] Wohlfahrt's experiments and those of Sander (which follow) indicate the characteristics of the psychological functions which are emphasized by the Leipzig laboratory, *i.e.* the nature of the original totalities, the feeling characteristics of these totalities, and the predispositional (*strukturelle*) factors in the operation and development of the psychological functions. To study the process by means of which *Gestalten* tend to be formed, Wohlfahrt presented, in a lightless room, small luminous line-figures and magnified them in successive stages until they were finally experienced as end-

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[22] A. S. Rogers, An analytic study of visual perceptions, this JOURNAL, 28, 1917, 550-558, esp. 576.

[23] C. H. Judd and D. J. Cowling, Studies in perceptual development, *Psychol. Monog.*, 8, 1907, (no. 34), 349-369.

[24] L. Hempstead, The perception of visual form, this JOURNAL, 12, 1900, 185-192.

[25] E. Wohlfahrt, Der Auffassungsvorgang zur kleinsten Gestalt, ein Beitrag zur Psychologie des Vorgestalterlebnisses, *Neue Psychol. Stud.*, 4, 1927, (cited by Sander, *op. cit.*, 57).

Gestalten. In the original stage the figures were found to be of a total, unitary character, undifferentiated and difficult to name. Then there occurred a circular contouring that continually shifted and changed about. The contours in this stage began to differentiate, but were always labile. The enclosed parts remained unarticulated for a long time and were characterized by a complex 'feeling-like' quality. Finally, they began to differentiate into regular forms. New stages were reached suddenly and the more and more detailed articulated parts grew out of the previous simpler totals. In this *Vorgestalt*-period the figures were enormously labile and were "filled with internal life and movement." According to Wohlfahrt's description, this lability increased in places to a pulling, pushing, or tearing, usually just before a new member broke off from the regular scheme. Only at the end-stage, with the preponderance of the stimulus-conditions, did the lability go over into a certain definiteness and quietness. Wohlfahrt concludes that development proceeds, first, by way of an increasing differentiation and articulation of details from the original unitary total, secondly, in a transition to stability from the originally labile and unstable state, and, thirdly, in a transition from figures with a richness of feeling-quality to those showing to the experiencing subject a greater coldness and objectiveness. Wohlfahrt, himself, does not separate the directions of variation in this three-fold way. He discusses them all as parts of the same process. But it is uncertain whether the fusing and identification of these modes of change are necessary.

With respect to the developmental changes in perception and upon the basis of Wohlfahrt's experiments and his own unpublished investigations, Sander makes the following statement. "Die Untersuchungen zur Aktualgenese von Gestalten sind in doppelter Hinsicht von Bedeutung: sie zeigen einmal, dass wie in der Phylo- und Ontogenese so auch hier psychische Ganze mit dem Merkmal der Gestaltheit sich entwickeln aus relative ungegliederten, in das Gesamterleben eingeschmolzenen Ganzen von gefühlsartigen Qualitäten. Diese gefühlsartigen Qualitäten sind nicht blosse Begleiterscheinungen des Gestaltprozesses, sondern funktional wesentlich als Werdensgrund. Ohne das zunächst gefühlsartige Erleben ganzheitlicher Zusammenhänge ist auch in höhern Schichten keine produktive Gestaltbildung möglich."[28] Sander says further that the dispositional structures of the organism tend in the direction of closedness and unitariness toward simplicity and regularity, and toward symmetry and "articulation in the preferred space-directions."

With the exception of the method of Judd and Cowling, the technique used for the purpose of obtaining progressive modifications in perception has resulted in progressive changes which may be a function of changes in the stimulating conditions. The results cannot therefore be accepted as giving valid information upon the course of the perceptive function in the adult organism. While the literature has raised certain problems upon which the present results may bear, it has furnished but little reliable information upon the specific problem here investigated.

[28] Sander, *op. cit.*, 59.

The Experiments

A series of 12 line-patterns was presented at 4 places in the periphery of the retina, one temporal (T) and one superior (S) position at 10° from the fovea, and one nasal (N) and one inferior (I) position at 22° from the fovea. Individual measurements were made in order to avoid the blind-spot. Forty trials with each of two *O*s were given at each of these four positions. In sixty supplementary trials with the same *O*s, and in sixty and seventy-five trials respectively with two additional *O*s, only two retinal positions, T 10° and 20°, were employed. Each of the 12 patterns in the series was presented at each of the explored retinal positions.

No pattern was presented twice in immediate succession. The sequence of presentation of the various pattern-groups was haphazard. When all four retinal positions were employed, the first three patterns (Patt. 1-3) were presented (say) at T 10°, the next three (Patt. 4-6), at S 10°, the third three (Patt. 7-9) at N 22°, and the last three (Patt. 10-12) at I 22°. In the next presentation of the series, Patt. 1-3 might be given at S 10°, Patt. 4-6 at N 22°, Patt. 7-9 at I 22°, Patt. 10-12 at T 10°, and so on. When only two retinal positions were employed, Patt. 1-6 were presented (say) at T 10° and Patt. 7-12 at T 20°. At the next presentation of the series, Patt. 7-12 were presented at T 10° and Patt. 1-6 at T 20°, and so on. A haphazard order also within the pattern-groups was always maintained.

The 12 patterns used (Fig. 1) were chance linear arrangements obtained by drawing the configurations which resulted in dropping from a height upon a plane surface three wires of unlike lengths. The selection of the twelve patterns to be used was made upon a number of variables of form and arrangement detected by preliminary foveal observations with trial *O*s who were not used in the main experiments.

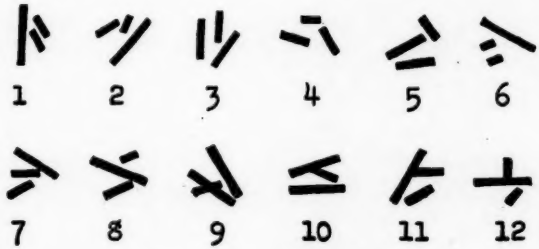

Fig. 1. Stimulus-Patterns

The apparatus consisted of the large Hellpach perimeter with its arm held stationary in a horizontal position and modified by the addition of a stationary vertical arm.[27] On the vertical and horizontal arms were placed four light-tight metal boxes

[27] W. Hellpach, *Philos. Stud.*, 15, 1898, 525 ff. Cf. W. Wundt, *Grundzüge der physiologischen Psychologie*, 2, (6th ed.), 1910, 189 f.

each containing a 5-watt lamp with rheostat-control. The distance from the stimulus-object to *O*'s eye was 1.09 meters. In front of a circular opening 1½-in. in diam. in each light-tight box, glass slides were placed. The slides were of colorless ground-glass and the patterns, cut from black gummed-paper, were pasted upon the slides. Over the patterns on the glass slide was pasted translucent white paper which permitted the pattern to be seen only by transmitted light. The lights in the boxes were controlled by a fall-apparatus with an exposure-time of 100 sigma ±1.3. The visual field, with the exception of the 1½-in. circular openings, was of neutral gray shade-cloth, 6 ft. high and following as a cylindrical surface the curved horizontal arm of the perimeter. The general illumination, provided by 4 field-lights, was weak daylight. The intensity of the general background, as measured by the Holophane light-meter, was 0.3 millilamberts and that of the stimulus-ground, 39.0 millilamberts. Weak daylight illumination was used to avoid shifts of fixation which easily result from the sudden appearance of a light upon a lightless ground, and also to avoid large changes of retinal adaptation. A small mirror, suspended by threads from the ceiling and floor of the room at half the focal distance, served as a fixation-point.[28] Biting-boards were used to aid fixation. The left eye of *O* was covered by an optician's patch. Where shifts in fixation were reported, in spite of our precautions, the pattern in use was discarded and reintroduced later in the series.

After *O* was seated in place and before the linear pattern was exposed, the following instructions were read.[29]

Draw and give full commentaries upon what you see. Avoid sets and consider each presentation upon its own merit without reference to any other."

O then fixated his own pupil in the mirror,[30] using the biting-board to keep his head in a steady position. After the signal "Fixate, ready" a trip on the pendulum

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[28] Described by K. M. Dallenbach, this JOURNAL, 42, 1930, 116.

[29] The observers were members of the staff of the Department of Psychology at Cornell University, C. W. Fox (*F*), A. D. Glanville (*G*), J. McV. Hunt (*H*), and Dr. R. B. MacLeod (*M*).

[30] The eyes of the *O*s were tested for errors of refraction and the following reports were received from the oculist, Dr. Albert C. Durand, Ithaca, New York.

M: Vision without drops: R = 20/50+; L = less than 20/150
with drops: R = 20/70 (lens: +sph. 0.50, —cyl. 1.75, ax. 169 = 20/15) ½° esophoria
(cycloplegia) L = less than 20/150 (lens: +sph. 1.25, —cyl. 5.00, ax. 25 = 20/40) 1° left hyperphoria
Media clear; fundi negative

G: Vision without drops: R = 20/15?; L = 20/20
with drops: R = 20/20? (lens: +cyl. 0.25, ax. 75 = 20/15) 1½° esophoria
L = 20/20?? (lens: —cyl. 0.50, ax. 120 = 20/15) ½° right hyperphoria
Media clear; fundi negative. No lenses ordered and none used in present experiment.

F: Vision without drops: R = 20/30+; L = 20/30
with drops: R = 20/30+ (lens: —sph. 0.25, —cyl. 0.37, ax. 100 = 20/15) 1° exophoria
L = 20/30 (lens: —sph. 0.25, —cyl. 0.75, ax. 75 = 20/15)
Media clear except for remains of pupillary membrane across right pupil; fundi negative. Lenses ordered but not used in the present experiment.

of the control-apparatus was released and the pattern was illuminated. *O* was asked to indicate on each drawing of the figure as seen the degree of certainty as to the reliability of his drawing and commentary.

RESULTS

The terms descriptive of the apprehended object employed by the *O*s may be classified under three headings; viz., characterization (1) of the linear parts or members of the figures as perceived (referred to below as *P*), (2) of the relations (R) of the linear parts to each other, and (3) of the total (T) figure as seen. More precisely stated, the characterizations are as follows:

(P) Apprehended linear parts
  (a) number
  (b) direction
  (c) length
  (d) width
(R) Apprehended relation of parts
  (a) to each other
  (b) dominance of any member
(T) Apprehended total figures
  (a) in terms of figure
    (i) integrity or unity of figure
    (ii) general direction
  (b) reference beyond the figure
    (i) affective tone
    (ii) dynamic character
    (iii) familiarity

Progressive changes in perception. Those authors who have discussed the possible directions of development in behavior and in psychological functions have suggested that these directions can be completely summarized by allowing for two types of change, (1) the increasing differentiation of an originally unitary total and (2) the integration of originally independent parts. An examination of the temporal sequences of our *O*s' drawings and reports shows, however, that neither of these specific directions of modification was found in the present experiments. The changes that did occur were mainly modifications in the perception from a labile, plastic, unstable beginning to a condition of stability.

In our results, the evidence for progressive change is of two kinds. The most striking evidence is provided by the sequence of drawings. The first drawings made by a given *O* with a given stimulus-pattern differ considerably from each other. But in the later drawings a stage is finally reached in which the drawings assume and retain a certain fixed form. Perception has become stabilized.

It appears from the drawings and the verbal reports that this final stabilization in the apprehended pattern has occurred for *G*, *F*, and *H*. For *G*,

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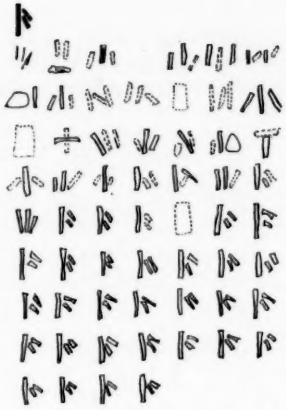
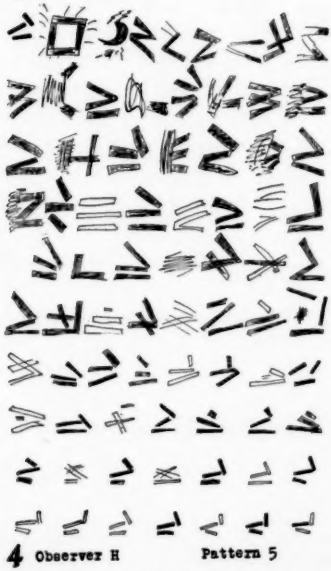
H: Vision without drops: R = 20/30; L = 20/20
Further tests not made

Our study has profited from the interest and the skill of an exceedingly competent ophthalmologist and oculist, who has appreciated the special difficulties in determining conditions in extra-foveal vision.

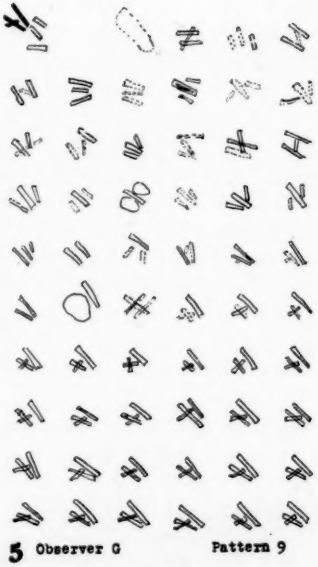
2 Observer H Pattern 1



3 Observer G Pattern 1



4 Observer H Pattern 5



5 Observer G Pattern 9

FIGS. 2–5. OBJECTS AS DRAWN BY *G* AND *H*

Patt. 1 retained its final form after the 28th presentation at 10° and after the 35th presentation at 20°; Patt. 5 the 34th (10°) and the 39th (20°); Patt. 9 the 39th (10°) and the 34th (20°). For *F*, Patt. 1 retained its final form after the 71st presentation (10°); Patt. 5 at 72nd (10°); and Patt. 9 at the 68th (10°). Very little was seen by *F* at the 20° and 22° positions. For *H*, Patt. 1 stabilized at the 63rd presentation (10°) and at the 64th (20°); Patt. 9 at the 61st (10°) and 63rd (20°); and Patt. 5 at the 65th (10°) and 68th (20°).

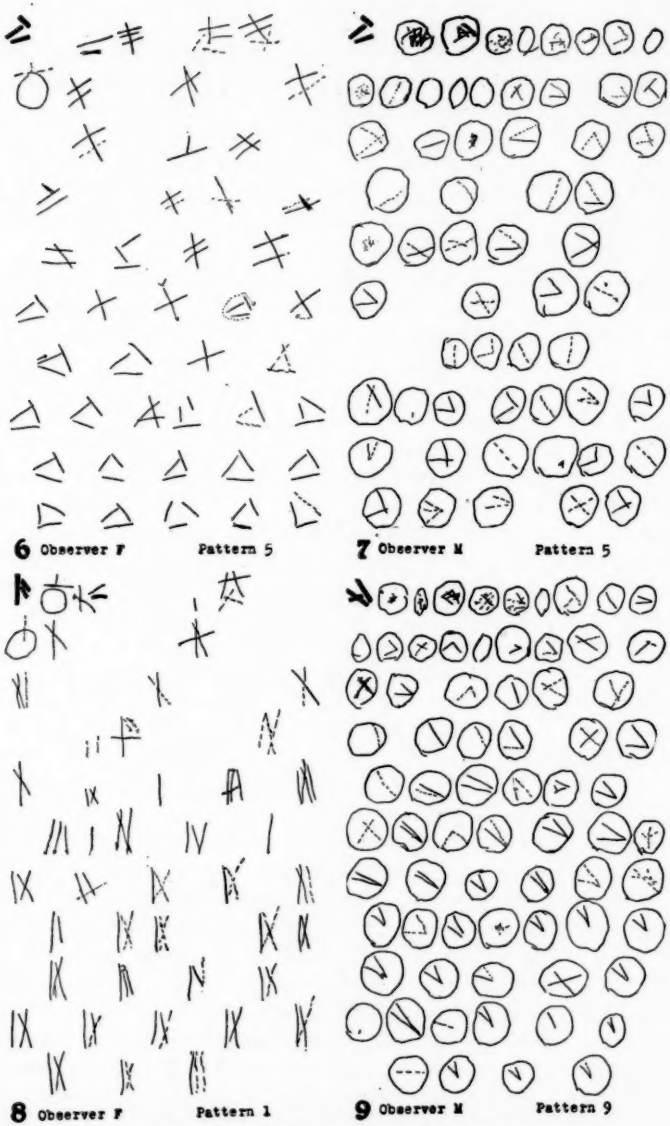
Typical progressive changes in apprehension are portrayed in Figs. 2-5, which contains the entire series of drawings made by *G* and *H* from the presentation of line-patterns No. 1, by *H* of No. 5, and by *G* of No. 9. In the upper left-hand corner of each of the four series of drawings stands the stimulus-pattern presented for 1/10 sec. at some peripheral part of the retina as described above. The successive drawings are to be read in lines from left to right beginning at the top.[31]

In Fig. 2, it is obvious that *H* first apprehended highly variable objects. In many of the objects occurred either three or four lines in arrangements suggestive of the line-pattern given, but with no fixed order. There is a tendency for the lines to converge at one end and to diverge at the other. Various spatial orientations occur. Twice *H* stylizes the object as 'K.' Several times the object is M-like. It is only near the end of the sixth horizontal line of drawings that *H* gives the essence of the line-pattern; a vertical accompanied by two down-right diverging lines. This then slips away, but returns, and is repeated again and again with slight variants. Here stabilization occurs. It is to be noted that the stabilized form is never wholly faithful to the light pattern presented to the eye.

In Fig. 3, *G* never draws the wide variety of objects that H draws. Nevertheless, it is obvious that the retinal stimulus-pattern is for a long time inadequate to a perception faithful to the pattern. Sometimes there is a complete blank. Sometimes there is great uncertainty (light broken lines in the drawings). But presently appears something-vertical-and-something-leading-off-downward. This something soon stabilizes and we have a succession of drawings which never 'copy' the line-pattern but always thereafter indicate its essential features.

Fig. 4 shows a progression very similar to that of Fig. 2. The organism's resources for perceiving are wholly inadequate but *H* struggles valiantly, soon hinting at the characteristics of the line-pattern. Again it is obvious

[31] The adequacy of each *O* in delineating an apprehended object by a linear drawing was determined by the reproduction of foveally-seen objects requiring the same kind and degree of skill in representation.



FIGS. 6–9. OBJECTS AS DRAWN BY *F* AND *M*

that the general orientation of the object is lacking and that angles, distances, and juxtapositions are loose and shifting. Gradually the details fall into their approximate places; but a detail is commonly found, only to be lost and later rediscovered. In the 7th and 8th horizontal lines the function is showing decided progress. The 9th line is almost stable, and the 10th approaches, with but minor variants, the 'final' object.

In Fig. 5, *G* runs true to his earlier performance (cf. Fig. 3), but with a new line-pattern, No. 9. The task is clearly of about the same degree of difficulty as before, and stabilization appears at about the same place. A close comparison of Figs. 3 and 5 will make it evident that in spite of *G*'s vague groping and high uncertainty throughout the twenty or more early trials, the two stimulus-patterns, Nos. 1 and 9, are exerting an unlike influence upon the organism, directing it toward unlike apprehensions. When it is remembered that these patterns are presented day after day and week after week on various peripheral areas and in a chance order for the entire set of 12 patterns (never in immediate succession), it is striking that the individual *O* should exhibit such a high constancy in the direction and the rate of his perceptive accomplishment.

Upon an alleged explication of an original totality, which might be expected gradually to rise from its ground and to acquire specificity, we have little affirmative evidence to offer in our drawings. There is here not much to comfort the *Gestalttheoretiker*. Our commentative reports do not offer much more. Verbal delineation in terms of parts, relations, and totalities (see above) present virtually the same ratios from the first observation of a given stimulus-pattern to the 60th, 70th, or 100th. Out of several thousand descriptive phrases, recorded along with the drawings, we can find no evidence that progress is, under the terms of our experiment, either a march from a vague totality toward a differentiated multiplicity or a march from unrelated dismembership to an integral unity.

Scarcely less instructive than our results with *G* and *H* are those with *F* and *M*. *F* failed to base any perceiving operation upon stimulus-patterns impressed at an eccentricity of 20° from the fovea. At the 10-degree positions he came to fairly stable forms, but with many failures and with 'final' forms which can scarcely be said to represent the delineations of the stimulus-patterns. Figs. 6 and 8 reproduce *F*'s drawings for patterns No. 5 and No. 1. In Fig. 6, a loose triangularity appears, and in Fig. 8 something like a "IX." The wide disparity between retinal input and psychological product may be due in part to dioptric defects. The art of dioptrics finds a great difficulty in defining the exact relations between the impinging light-pattern and the resulting stimulus-pattern where the light falls

obliquely upon the eccentric retina. Foveal vision is much more easily and correctly defined by oculist and optometrist. There may have been unusual dioptric distortions of the pattern in the eyes of *F*. Whatever the inadequacy of these primary conditions of perceiving may have been, however, the fact of stabilization in this *O* is clarified by his comment that he had a fixed expectation (self instruction) of some familiar form, and that when such a form appeared (*e.g.* the triangle or the IX above) he regarded the perceived object as final and so continued to draw minor variants of this form. The common illusory distortions of perceived objects under expectation or disturbing emotion are well authenticated even where the conditions of perception are much more nearly adequate than in the cases cited.

The perceptive functions of *M* also call for special remark. An inspection of Figs. 7 and 9 will make this clear. Finding it exceedingly difficult to 'see' anything in the circular lighted area, *M* reported many blanks and often simply outlined the circle and inserted vague lines or dots within it. No certain progress toward such forms as vaguely represented for *F* the stimulus-patterns now appear. The unlike inclinations of patterns No. 5 and No. 9 (as here presented) are scarcely to be recognized in *M*'s drawings. It is probable that ocular imperfections again play a part. We must note, however, a fixed comment of M. Conviction that his perceptive resources were not up to the task early led him toward discouragement and to a self-instruction of anticipated failure to perceive clearly and adequately. Here the vague and formal drawings, which tend to become stereotyped, probably represent rather an honest attempt to perform a task than the natural progress of a psychological function under the guidance of stimulus. The results of *F* and *M* suggest, then, the deflective effect of a dominating self instruction. Wanting the report of this diverting cause, the results would have to be referred (as they usually are in such cases) to the broad category of "individual differences."

In our historical section, researches were cited which had found either 'affectivity' (Wohlfahrt, Sander) or 'familiarity' (Philippe) to be a characteristic mark of perceiving. We therefore were at pains to control these factors. If either of them played a part, we should expect these 'feelings' to stand related to our process of stabilization. Such things as 'disturbing,' 'pleasing,' and 'displeasing' appeared in the reports of only one of our *O*s (*G*), and here almost without exception before the stage of stabilization appeared. They are obviously related to the unsatisfactoriness of the apprehended object under the demands of a self-appointed task or *Aufgabe* and not to the progress toward stabilization. Nothing like the 'internal life

and movement' of Wohlfahrt's descriptions came out of our observations. These alleged 'dynamic' qualities may be a function of the materials used or (as seems to us more likely) of self and occasional instructions. The leveling, emphasizing, assimilating, and changing-toward-type of Wulf, Gibson, and Philippe may occur in repeated perceptions of certain conventional designs and objects, under certain instructional tasks, but we found no trace of such changes in our simple apprehension of unknown patterns. That curious phenomenon called 'familiarity' was often reported by *G* and *H;* but it came both in the early stages and in the later, *i.e.* both before and after stabilization had been attained. The mark of familiarity attaching to the object perceived stood, therefore, in no specific relation to the attainment of the stable stage of apprehension.

With the Zigler type of conventional object, quite a different result might well be expected. Our non-conventional and 'unknown' patterns, on the other hand, lent little incentive to look for, or to recognize, familiar figures, which might then have become 'stabilized' and invariable.

SUMMARY

The study has attempted to determine and to record successive changes of a functional sort in perceiving. In order to produce these changes in the simplest way, unknown and unconventional collections of three short lines of varying lengths and arrangements were used as a stimulus-pattern upon the peripheral retina. Under short exposure (1/10 sec.) perception was at first vague, indeterminate, and uncertain. With many repetitions, however, the perception suffered notable changes.[32] The initial stage was followed by hints in the perceptive product of the line-members, spatial orientation, and internal relations of the stimulus-pattern. After gains, shifts, losses, and regains in these particular moments, the processes and the products of perception became stabilized and certain. Whether the stable object represented the stimulus-pattern or sharply diverged from it, it was maintained with slight variations until the final exposure of the appropriate light-pattern. Since this progression appeared for each stimulus-pattern, although 12 unlike patterns were carried through together in a

[32] After describing the temporal changes which occur in habituation, Bentley insists that, "while we think most naturally of the consolidating changes which we have just now discussed as referring to action, and especially to those action-trains which give us skill, it is important to observe that every one of the psychological functions passes through, under its own exercise, such modifications as we have described. No mode continues just as it began. And this fact is the beginning of those progressive changes in the human being which we call, when taken change-by-change, learning, and, when taken all together, psychological development." *The New Field of Psychology: Part i, The Psychological Functions,* 1933, 64.

chance order which never permitted the immediate repetition of any single light-pattern, it seems likely that the orderly changes described represent fundamental stages in the growth or development of a fairly simple function in the human adult under simple and constant receptorial conditions and with carefully controlled instructions. The results supplement other studies of perception which have employed known and conventional object-stimuli and which have therefore dealt with more complex and more variable conditions both within and without the organism. Our results offered no support to either of those alternative theories which regard perceptive apprehension as progressing from (1) an initial undifferentiated totality to a diverse multiplicity or (2) from an initial inchoate multiplicity to a well-integrated configuration. In our experiments, the main progress was from a labile, uncertain, and inconclusive function to a fixed, stable, and conclusive performance whose end-product was a determinate and abiding object.

A RECIPROCAL REFLEX SYSTEM IN THE ACCOMMODATION-CONVERGENCE RELATIONSHIPS

By Edwin Forbes Tait, University of Pennsylvania

For many years there has been considerable speculation among workers in psychological and physiological optics regarding the relationship between the functions of accommodation and convergence in preserving clear single binocular vision. Helmholtz advanced the theory that these functions normally go hand in hand and that they are, in their mutual relationship, the manifestations of a built-up association not belonging to the class of congenital involuntary concomitant movements.[1]

Interest in the problem was lacking for many years and it remained without experimental investigation until the general development of visual psychology and physiology brought it to the attention of Sheard,[2] who presented, in 1917, the problem in a concise and systematic manner for the first time. Sheard recognized the importance of the psychological aspects of the problem and began his work by attempting to reduce the judgmental errors inherent in the older purely subjective methods by the development of a more objective system for the determination of the accommodation and convergence values. The methods then developed are the basis of those used in the present investigation. So far, however, it has been impossible to utilize wholly objective methods and thus the present investigation assumes a considerably more psychological than physiological character.

A fairly complete citation of the meager literature up to 1920 will be found in the paper by Sheard.[3] As a result of the work reviewed, Sheard comes to the conclusion that 70% of persons possessing low refractive errors, but with otherwise normal eyes, show from 2–7 prism diopters of accommodative exophoria at a fixation-distance of one-third meter, and he further concludes that the optimum condition is one where there is from 4–5 prism diopters at that distance.

When binocular vision at near is required, as in reading, it is evident that the visual axes must converge toward the point of fixation. The amount of accommodative exophoria represents the amount of that convergence which is supplied by the fusion faculty in the interest of single binocular vision, but as this usually represents only about one-fourth of the whole, the major part of the convergence

*Accepted for publication January 16, 1932. From the Psychological Laboratory of the University of Pennsylvania.

[1] H. von Helmholtz, *Handbuch der Physiologischen Optik*, 3rd. ed., 3, 1910, 48, 51.

[2] Charles Sheard, *Dynamic Ocular Tests*, 1917.

[3] Sheard, On accommodative exophoria, *Amer. J. Physiol. Opt.*, 1, 1920, 234.

present in normal cases must therefore be due to the convergence innervation stimulated by the use of the accommodation and designated in this paper as accommodative-convergence.

Weymouth, Brust, and Gobar,[4] in 1925, support Sheard's conclusions and add somewhat to the historical background.

Ames and Gliddon,[5] using an adaptation of the Pereles and Hess apparatus, as described by Howe,[6] have shown that the actual accommodation used in reading rarely corresponds exactly to the fixation-distance, due to the ability of the visual perceptive centers to resolve retinal images even when they are formed by multiple diffusion circles. For a fixation-distance of one-third meter, however, they found the accommodative lack to be only from about 0.08 to 0.25 diopters. Values of this order, important as they are in a survey of accommodative phenomena, may safely be ignored in this investigation as they are considerably less than the expected experimental error. For all practical purposes, therefore, the amount of accommodation required for a fixation-distance of one-third meter may be considered as three diopters.

All the preceding investigations, however, are concerned either with the characteristics of accommodation or with the effect produced upon the convergence by the exercise of accommodation or accommodative effort. The possibility of a comparable or reciprocal reflex wherein accommodation may be the result of convergence or convergent effort has not heretofore been considered.

The object of this investigation is to establish the presence or absence of reflex accommodation due to a convergence stimulus, and if it apparently is present, to ascertain its values and characteristics as far as possible. It is obvious that the only approach is from a combined objective-subjective standpoint, obtaining, independently, the convergence value subjectively and the accommodative value objectively.

If, by suitable methods, the accommodation resulting from the convergent impulse, if any such be obtained, be measured and compared with the amount of convergence present when the subject is accommodating, with the fusion faculty non-operative, any positive correlation between the two would support Helmholtz's theory of a true association relationship, built up as the result of simultaneous use of accommodation and convergence in the early years of life.

No objective procedure would be sufficiently valid in the measurement of convergence due to the fact that the point upon the cornea through which the visual line passes and upon which an objective determination must depend cannot at present be determined accurately. The amount of

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[4] F. W. Weymouth, P. R. Brust, and F. H. Gobar, Ocular muscle balance at the reading distance and certain related factors, *ibid.*, 6, 1925, 184-205.

[5] A. Ames and G. Gliddon, Ocular measurements, *Presessional Vol. Sec. Ophth., Amer. Med. Assn.*, 1928, 11-47.

[6] Lucien Howe, *The Muscles of the Eyes*, 1907, 329-330.

accommodation, however, can best be determined objectively as skiametry affords the most accurate means with which to calculate the total refraction at any given distance.

Skiametry, as used for many years by refractionists, is carried on by the use of a skiascope or retinoscope, which, in its simplest form, is an adaptation of the ophthalmoscope and consists essentially of a small round mirror with a sight hole in its center. When the light from a suitable source of illumination is reflected into the eye of the subject, the observer, peering through the aperture in the mirror, observes the illumination in the subject's pupil and notes that if he tilts the mirror slightly the pupillary illumination apparently shifts in a direction either coinciding with, or opposite to, the movement of the incident light. Meanwhile the attention of the subject is directed to a suitable fixation-object, which may be at any distance, and lenses are placed in front of his eyes so that the reflex movements become extremely rapid and eventually are completely neutralized. This point of neutralization occurs when the subject's retina, which is here acting as a secondary source of illumination, is conjugate with the principal nodal point of the observer's eye. Knowing the distance between the skiascope and the subject's cornea, the lense value in diopters can easily be calculated.

Most modern skiascopes have a self-contained illuminating mechanism, but are used in precisely the same manner as the simpler type. Dynamic skiametry is skiametry carried on while the subject is converging to, and fixing upon, a near object. This modified system, first described by Cross,[7] and developed further by Sheard,[8] and considered by the latter as the most accurate existent method of investigating the negative relative accommodation, presented little difficulty in application.

With an emmetropic, or normal eyed, individual or one made so by the proper corrective glasses, there must be approximately 3 diopters of accommodation exerted in order to see small print distinctly at a distance of 1/3 m. If fixation is constant but the accommodation varied by the introduction of convex lenses, there will be some relaxation of accommodation in most cases, but rarely will the individual, if under the presbyopic age, accept the full 3 diopters of convex lens power which would relax the accommodation completely. Usually there will be, as shown by clinical experience, a greater or lesser amount of accommodation retained, apparently due to the presence of convergence for, when the latter is relieved by the application of prisms base-in, the full convex value of 3 diopters is usually promptly accepted.

In an attempt to determine the accommodative status subjectively, however, acceptable judgments as to the point of beginning blur are difficult to obtain, probably due to the ability of the visual centers to resolve blurred retinal images. In the experience of the author, objective determinations of the refractive, and therefore accommodative, status may be carried on by the aid of dynamic skiametry with a possibility of error not exceeding ½ diopter. In addition this procedure permits the use of sufficient initial convex lense power to promote ciliary muscle relaxation to the point of the convergent-accommodative innervation. The differ-

[7] A. J. Cross, *Dynamic Skiametry in Theory and Practise*, 1911.
[8] Charles Sheard, *Dynamic Skiametry*, 1920.

ence between the spherical lense value found by dynamic skiametry and that accepted subjectively by the subject would, when subtracted from the 3 diopters necessary for focussing at 1/3 m., be equal to the amount of accommodation stimulated by the convergence to, and fixation upon, the near-point fixation-object.

It is possible to measure the amount of accommodative convergence used by an individual under given conditions of near-point fixation by dissociating the fusion faculty by the application of a prism with the base down in front of one eye and then bringing into vertical coincidence by measured prisms the diplopic images produced by such a procedure. The base-in prism value necessary for coincidence represents the accommodative exophoria provided that the distance ocular muscle imbalance, if existent, has been previously corrected. The total convergence of the visual axes from parallelism to fixation at 1/3 m. may then be calculated in accordance with the formula developed by Weymouth, Brust, and Gobar.[9] The accommodative convergence may then be calculated by subtracting the accommodative exophoria from the total convergence as found by the above formula. This method of calculation, developed by the author, avoids the exceedingly laborious graphic method suggested by Maddox and quoted and amplified by Sheard.[10]

Subjects. The results of a quantitative study of 500 individuals, by methods developed in the study of the foregoing considerations, are here presented. All of the *Ss* were between the ages of 10–40 yr.—not less than 10 yr. in order to insure reasonable accuracy in the subjective determinations, and not over 40 yr. due to the desirability of avoiding the lessened accommodative amplitudes which usually become apparent after that age. All had, also, single binocular vision, visual acuity of 6/6 with the proper correcting lenses, and, in cases where visual defects were present, wore the proper correcting lenses. An arbitrary division into 4 age-groups was made in order to afford, as far as possible, a basis of comparison between relatively older and relatively younger groups. The majority of the *Ss* were college and high school students, but, as will be seen from the tabulations, a sufficient number of older persons was included in order that the results might not represent the conditions prevailing among the younger individuals only. The *Ss* were selected from among a considerable number of persons who had reported to the author for routine examinations for visual efficiency after having previously been examined for visual defects.

All the *Ss* were entirely uninformed as far as the problem was concerned, considering the procedure simply as part of the routine procedure for the determination of visual efficiency. The results have been considered collectively so that there was no need to identify the *Ss* in any way except by numbering them successively from 1 to 500.

In selecting *Ss*, care was taken to eliminate from consideration the few cases in which there was a manifest condition of eye-strain due to uncorrected ametropia, muscular anomalies such as convergent squint or hyperphoria, and where the astigmatic correction was not consistent or varied during the investigative procedure.

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[9] F. W. Weymouth, P. R. Brust, and F. H. Gobar, Ocular muscle balance at the reading distance and certain related factors. *Amer. J. Physiol. Opt.*, 6, 1925, 189.
[10] Sheard, *Dynamic Ocular Tests*, 1917, 60.

Apparatus. The apparatus consisted of a standard DeZeng phorometer, equipped with a near point target; an ordinary ophthalmic trial case; a test cabinet for visual acuity judgments and fixation; a self-luminous dynamic skiascope, and a 10-cm. rule graduated in mm. The phorometer was supported by a suitable floorstand and contained, before each eye, 3 cells for the reception of the trial case lenses and a rotary prism calibrated to read from 0–30 prism diopters. The rotary prism is attached in such a manner as to make possible the placing of the prism base-apex line in any given direction. In addition there is a white near-point card, measuring 13 cm. square and with a 4 mm. black E printed in its center, held upon a rod at a distance of 1/3 m.

The ophthalmic trial case used for the phorometer cells is composed of convex and concave cylindrical lenses, and is so arranged that any given lense power may be obtained by one or a combination of two or more lenses. It is calibrated in the familiar lense dioptric system.

The test cabinet used was the 20th Century Cabinet produced, like all the other items of apparatus, by the American Optical Company. Used at a distance of 6 m., for which it is calibrated, its fixation-letters are calculated to subtend a 5′ angle at their designated distances. The increasing size of the letters gives successively 6/6, 6/7.5, 6/10, 6/12, 6/15, 6/20, 6/30, and 6/60, in terms of visual acuity. The numerator represents the fixation-distance while the denominator expresses the smallest letters seen clearly at that distance. Thus 6/12 visual acuity means that *S* cannot see clearly letters smaller than the 12 m. group at a distance of 6 m.

The last of the series, the 6/60 object, consists of the one letter, E, and was the stimulus used for fixation in determining the presence and amount of esophoria or exophoria for distance. The smaller letters were used for the judgment of visual acuity with 6/6 considered as the optimum amount.

For the skiametric procedure, the skiascope used was the Tait dynamic instrument, developed by the author, and described fully in a previous paper.[11]

Procedure. The procedure was identical with every *S* regardless of age. The name and age of *S* were first noted and then the inter-pupillary distance was measured by the parallax method with the millimeter rule. This distance, ascertained when each eye was fixing straight ahead, is obviously equal to the distance between the centers of rotation of the two eyes which is the basis for the total convergence calculation. Dynamic skiametry was the next procedure. With sufficient convex lense power before each eye to make *S* artificially myopic for the fixation distance, his attention was directed to the skiascope target on the side opposite to that of the eye upon which *E* was working. The fixation-target to which *S*'s attention was directed, was previously illuminated by the chart illuminating device which is part of the instrument. *S* was then given the following directions:

Look constantly at the illuminated target on the instrument even if you do not see it clearly. Tell me if at any time you see two targets.

[11] E. F. Tait, A quantitative system of dynamic skiametry, *Amer. J. Optom.*, 6, 1929, 669-693.

By tilting the skiascope and observing the fundus reflex, the movement of the margin of the reflex was then noted and if it was in the direction opposite to the movement of the light upon the face, the convex lense value was reduced until the movement was extremely rapid, showing that the conjugate focus to *S*'s retina was approximately coincident with the position of the principal nodal point of *E*'s eye. If the initial movement coincided with the facial illumination, higher convex lenses were applied until the movement was in the opposite direction. As long as the movement of the reflex was rather slow, the lense values were successively reduced in each eye, shifting *S*'s attention to the right and the left targets successively, but as soon as the movement became rather rapid the attention of *S* was directed to the illuminated chart to his left and the right eye was then carried to the point of extremely rapid movement of the reflex. Then the skiascope illumination was shifted to the chart on the other side and the left eye corrected in the same way and to the same extent. Lack of fixation by both eyes was readily noticeable by reason of the rapid changes in the reflex or by *S*'s report of diplopia. The values thus found were recorded as the dynamic skiametric results. Shifts in the rate and type of the reflex movements were readily observable with lense differences of 0.25 diopter so that this figure would represent the maximum experimental error.

A careful subjective judgment procedure, with fixation at 6 m. and utilizing the same principle of supplying excess convex lense power and then reducing, was the next operation. One eye at a time was occluded and the highest plus lense with which 6/6 or better vision was attainable was noted as the subjective finding. In the vast majority of *S*s the astigmatism, if present, required for correction the same cylindrical value in the subjective procedure as in the dynamic, and only such cases are here presented, as those having variable cylindrical values introduced a consideration foreign to the purpose of this paper, that of compensatory lenticular astigmatism.

For the determination of the esophoria or exophoria at 6 m., a six-prism diopter prism, with the subjective lense correction in place, was inserted base down in front of the right eye and *S*'s attention directed to the 6/60 letter E in the test cabinet. *S* was then questioned as follows.

You now see two Es off in the distance. Is the lower E directly under the upper one, or is it to the right or to the left?

The rotary prism was then swung into place in front of the left eye with its base-apex line in the proper direction, and *S* given the following instructions.

Tell me just when the lower E is directly under the upper one.

If the rotary prism, when vertical coincidence was secured, is base out, esophoria was present; if base in, exophoria, and this and the amount were noted.

The same procedure was then followed with fixation at 1/3 m. on the near-point fixation-target and the esophoria or exophoria for near determined. During this process sufficient time, usually 1–2 min., was permitted for the lower E to settle into position as it usually oscillated somewhat while the prism value was slowly changing.

Discussion of method. It may readily be seen that the dynamic determination corrected not only *S*'s refractive error but that between the lenses and his accommodation the added positive refractive power for the near-point fixation was supplied. If the distance correction as found by the subjective method is then com-

pared with the dynamic skiametric findings, the amount of accommodation used during the dynamic procedure may be calculated readily by subtracting from 3 diopters the difference between the two.

That the stimulus for this accommodation is not due to the lack of a clear retinal image with less accommodation is apparent when the details of the dynamic method are considered. Sufficient convex lense power to relieve accommodation totally was present during the early phases of the operation, but was not utilized by the eye. It can be shown by inhibition with base in prisms that this residual accommodation is due to the convergent stimulus and that it, when present, is an indication of the efficiency of the convergence-accommodation reflex.

In the study of convergence it has been necessary to utilize the classification of ocular muscle anomalies, suggested by Stevens,[12] wherein the terms esophoria and exophoria serve to classify the possibilities in horizontal heterophoria. Esophoria is considered as indicating a tendency, while the fusion faculty is functioning to neutralize it and prevent actual movement, of the visual axes of the two eyes to converge with respect to each other. Exophoria, on the other hand, indicates a tendency toward divergence of the visual axes under the same conditions. Both esophoria and exophoria are necessarily relative to the distance of fixation and to the influence of accommodation. In the determination of either of these conditions the first requisite is to suspend the action of the fusion faculty and thus destroy its power to innervate the extra ocular muscles in the interest of single binocular vision. This procedure, accomplished by the use of one of the many dissociation methods, in the present instance by the use of a six-prism diopter prism base down in front of the right eye, permits the visual axis of each eye to assume a definite position determined by the extent and distribution of the reciprocal tonic innervation to the extra ocular muscles; provided that the fixation is at distance and any refractive defects are corrected by proper glasses. Where accommodation is in use, as in near fixation, more or less convergence effect stimulated by the accommodative action is usually added to the basic tonic innervational distribution, thus effecting a difference in the angular relations of the visual axes from those obtaining at distance. The effect of this accommodation can be measured without difficulty by comparing the results of the distance convergence determination with those of the near. By reason of the slight judgment error and the lack of accurate calibration of the rotary prisms to less than one prism diopter, there was an inherent maximum error of that amount in measuring esophoria or exophoria. Weymouth, Brust, and Gobar's table was therefore used only to the nearest full prism diopter value when calculating the total convergence from parallelism to the visual axes to fixation at 1/3 m. from the corneae.[13] Following are the total convergence values for pupillary distances of from 55–70 mm.

Pupillary Distance (in mm.)	Total Convergence (in prism diopters)
55–58	16
59–61	17
62–65	18
66–70	19

[12] G. T. Stevens, *Motor Apparatus of the Eyes*, 1906, 215.
[13] Weymouth, Brust, and Gobar, *op. cit.*, 190.

Convergent Accommodation (In diopters)	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
2.00										1		2	1			1						
1.75									1		1	4	2	1		2						
1.50					1		2	2	1	2	4	9	20	13	17	11	7	6	7	1	2	2
1.25			1		1		1	1	3	8	13	9	11	4	11	10	9	3	2	1		
1.00				1	1	2	5	12	12	5	18	10	9	5	6	4	5					
.75				1	1	8	16	8	11	10	7	7	2	3		1		2		1		1
.50	1	1	3	1	8	7	4	6	10	3	4	3	4		1		1					
.25	1	2	2	3	5	3	2	4	2		2			1								
0	11	6	2	3	2		1	1														

Accommodative Convergence (In prism diopters)

Fig. 1. Distribution of 500 Ss, Ages 10–40 Yr.

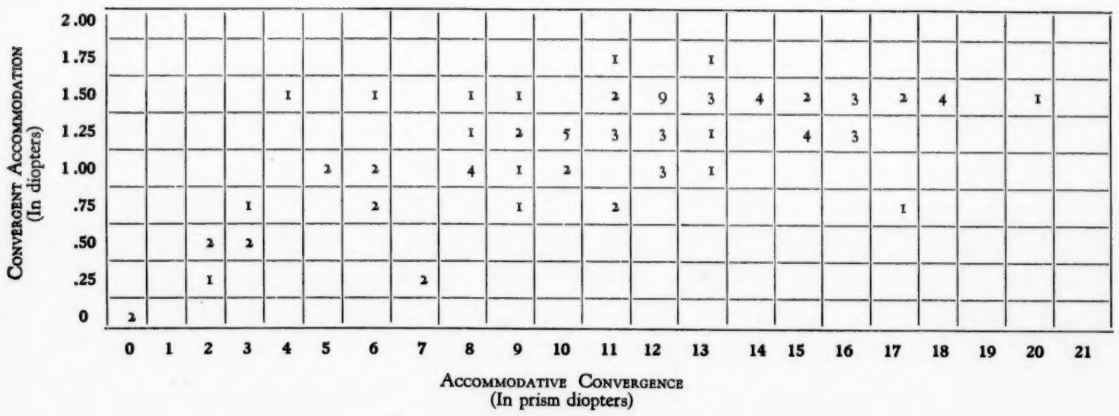

Fig. 2. Distribution of 89 Ss, Ages 10–17 Yr.

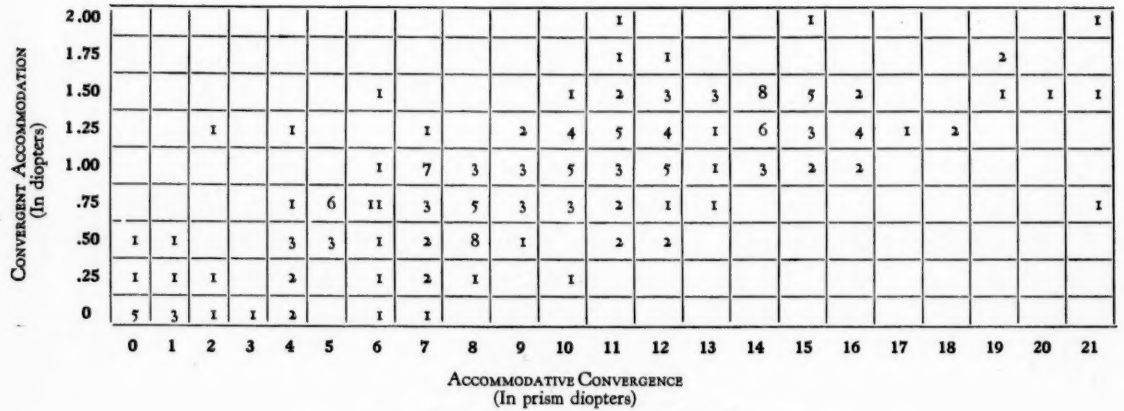

Fig. 3. Distribution of 190 Ss, Ages 18–25 Yr.

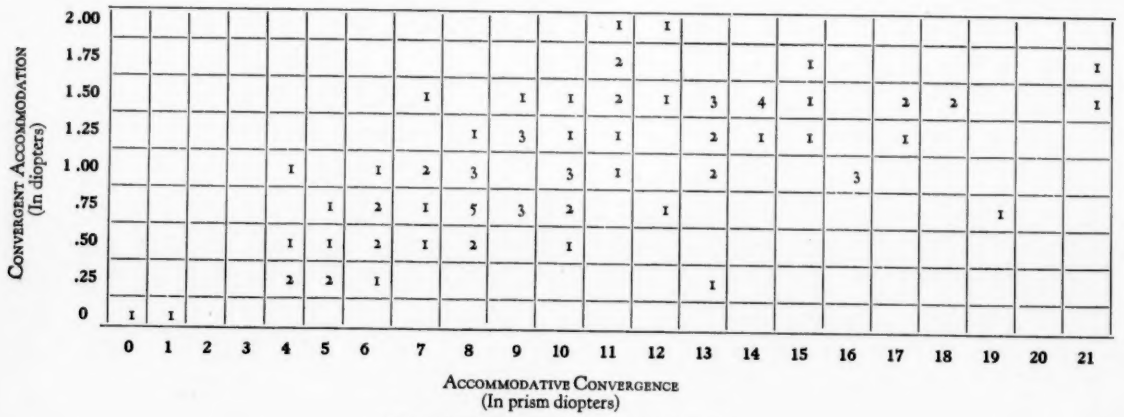

Fig. 4. Distribution of 84 Ss, Ages 26–30 Yr.

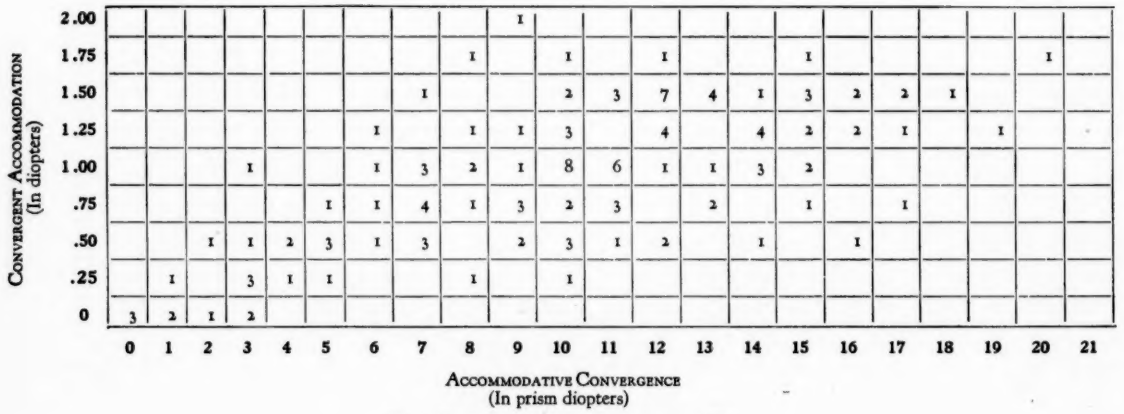

Fig. 5. Distribution of 137 Ss, Ages 31–40 Yr.

For purposes of calculation esophoria was considered as a plus quantity and exophoria as minus. The accommodative-convergence was then calculated by substituting in the following formulae:

$$(1)\ N_e - D_e = P_e$$
$$(2)\ C_t + P_e = C_a$$

Where:

N_e = Near point esophoria or exophoria,
D_e = Distance esophoria or exophoria,
P_e = Physiological exophoria or esophoria,
C_t = Total convergence from parallelism to one-third meter, and
C_a = Accommodative convergence.

Results

The distribution of the 500 cases with respect to their accommodative convergence and convergent accommodation value will be found in Fig. 1. In this whole series the greatest amount of convergent accommodation encountered was 2 diopters and the greatest amount of accommodative convergence was 21 prism diopters with 5 *Ss* each. On the other hand, there were 11 *Ss* with neither accommodative convergence nor convergent accommodation. The other *Ss* were distributed with noticeable regularity between these two extremes. A superficial inspection of the table will show that convergent accommodation is not only present in most cases but that there is a very positive correlation between it and the accommodative convergence. The actual coefficient of correlation, by the Product-Moment Method,[14] is $+0.73 \pm .014$.

The distributions of the 4 age-groups will be found in Figs. 2-5. Fig. 2 gives the results and scatter of the pre-college group, ages 10-17 yr.; Fig. 3, of the college group, ages 18-25 yr.; Fig. 4, of the group between the college and the pre-presbyopes, ages 26-30 yr.; and Fig. 5, of the pre-presbyopic group with diminished amplitudes of accommodation, ages 31-40 yr.

It will be noted that age has apparently no modifying effect upon the presence of either accommodative convergence or convergent accommodation as the general character of the distribution is similar in all 4 age-groups, the atypical as well as the typical cases being represented in each. In considering the atypical aspects of the distribution, it should be noted that there are relatively few cases in which there is accommodative convergence without convergent accommodation or convergent accommodation without accommodative convergence. A noteworthy characteristic of the convergent accommodation distribution is that, while there are 107 *Ss*

[14] H. E. Garrett, *Statistics in Psychology and Education*, 1926, 163-168.

in the 1.50 diopter group, there are only 15 *Ss* in the 1.75 diopter group, 6 in the 2.00 diopter group, and none higher. Between 0 and 1.50 diopters, however, is a steadily increasing number for every 0.25 diopter increase. This would give a badly skewed curve if plotted according to the amount of convergent accommodation.

The curve for the accommodative convergence would, on the other hand, show a very definite regularity if allowance were made for the inequalities introduced by the apparent predilection of the observer for even numbers in the choice of values made necessary by the lack of fractional sub-divisions on the rotary prisms.

Discussion. In view of the fairly high positive correlation obtained between the accommodative convergence and the convergent accommodation determinations, it is apparent that Helmholtz's view is supported and amplified in so far as that is possible with the relatively limited number of *Ss* studied. If we consider that the laws of association, as deduced from learning experiments, are valid in subliminal conscious processes as well as in the conscious (and there is no good reason for not doing so) the results of this investigation may be taken as indicating that there is usually a reciprocal relationship existing between accommodation and convergence, the source and type of the stimulus depending upon whether the accommodation or the convergence is the dominant factor. In this investigation, the author first made the convergence dominant by securing binocular fixation at a near-point and eliminating accommodation as far as possible by supplying convex lense power equal to the focal length of the fixation-distance. Under these conditions some accommodation did relax and was measured by the amount of plus lense value which had to be deducted from the initial plus excess.

Dominancy was then transferred to the accommodation by permitting the eyes to use a normal amount of accommodation and at the same time eliminating the influence of the fusion faculty, thus freeing convergence. The convergence existing under such conditions must be that which is stimulated by the presence of accommodation.

Binocular fixation and accommodation for a near point, however, if the values obtained in this experiment may be taken as representative, involve more of both accommodation and convergence than the usual amounts supplied when either is the stimulus and the other the effect. Inasmuch as accommodation is the primary requirement for clear vision, it is probable that the usual dominancy is that of the accommodation, which would give more or less accommodative convergence depending upon the efficiency of the reflex. This is the more likely in that there is a very effective mechanism in the fusional convergence innervation for supplying any de-

ficiencies in accommodative convergence. The convergent accommodative reflex probably would not, therefore, be effective under ordinary conditions, but would be immediately operative in situations where the convergence would become dominant as in the so-called 'fogging' system of subjective or objective refraction where strong convex lenses are applied at the beginning of the process.

The accommodative convergence values found in this experiment show that 8–14 prism diopters may be considered as the usual and therefore optimum range, thus agreeing very well with Sheard's determination.[15] It is also further apparent that convergent accommodation of over 2 diopters is not common, no such case being found during the present investigation. The relatively great number of *Ss* with 1.50 diopters, however, suggests that this amount represents the optimum value for this reflex. That the amount of convergent accommodation is not dependent upon the amplitude of accommodation of the individual is quite apparent when the values in the 4 age-groups are considered. The average amplitude of accommodation in the pre-presbyopic group, in which the amplitude is decreasing rapidly, must be much less than in any of the other groups, but an inspection of the age-group results will show that there are proportionally as many in the upper classifications in the older as in the younger *Ss*. This optimum value of 1.50 diopters for the convergent accommodation confirms quite fully the results of Bestor who found, as the result of a survey of 139 cases, 1.50 diopters as the average amount of accommodative lag with fixation at 1/3 m.[16] Although not so designated or considered by Bestor, the difference between the accommodative lag and the total near point refractive value of 3 diopters must have been due to the presence of that amount of convergent accommodation.

The existence of a reflex relationship between accommodation and convergence, reciprocal in nature, in which either can be the primary source of the stimulus and the other the response, may be tentatively established as a result of the work reported in this paper. The development of physiological optics in general and visual psychology in particular will probably demand further investigations along these lines.

Summary

(1) Accommodation apparently due to a stimulus primarily derived from convergence was present in practically all of the *Ss* and must therefore be considered as a normal reflex.

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[15] Sheard, *Op. cit., Amer. J. Physiol. Opt.*, 1, 1920, 249.

[16] H. M. Bestor, The interpretation of dynamic skiametric findings, *Amer. J. Physiol. Opt.*, 1, 1920, 229.

(2) The amount of convergent accommodation varied from 0–2 diopters with 1.50 diopters as the probable optimum value.

(3) The accommodative convergence values are in close agreement with those found in previous experiments.

(4) The amount of convergent accommodation in the average *S* is normally quite proportional to the amount of accommodative convergence, when measured by the methods used herein, as is shown by the relatively high positive correlation between them.

(5) The view is here suggested that there is a reflex relationship between accommodation and convergence, reciprocal and associative in nature, in which either can be conceived as the primary stimulus and the other the response.

INSIGHT *VS.* TRIAL-AND-ERROR IN THE SOLUTION OF PROBLEMS

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The exact nature of the mental and neural changes which occur whenever an act is learned continues to be a matter for dispute. Undoubtedly the issue will remain controversial as long as psychologists are persuaded that the real key to behavior is to be found in the properties of the central nervous system. The ultimate substantiation of any thorough-going theory of learning is admittedly contingent upon advances in neurological research; nevertheless, despite the reciprocal benefits which these fields have enjoyed, the learning hypotheses have always suffered from inadequacies in descriptions of the way in which an organism modifies its conduct when confronted with new situations. These deficiencies are not present in reports of single studies but appear in comparing several experiments because different problems involving distinct reactions were used.

An explanation of learning in terms of lowered synaptic resistance is plausible when built upon evidence derived from simple maze-running and the memorization of a nonsense syllable series where 'gradualness' in obtaining mastery is the most important single feature; but it rings less convincingly when applied to rational puzzle-solving in the human where 'suddenness' in making the correct response is outstanding. The 'trial-and-error' explanation of traditional associationism fits the typical negatively-accelerated curve very well, but the 'insight' version of Gestalt psychology is more in conformity with those curves which reveal precipitous drops or rises.

Configurationists maintain that trial-and-error accounts are valid only in those experimental situations where a perception or comprehension of the total setting is impossible under the conditions imposed upon the learner. There, accidental or chance happenings in the strict mathematical sense are the major determinants of progress. Whatever intelligence the learner may have is hamstrung by these mechanical limitations. In cases of true learning, *i.e.* instances where a survey of all the factors in the problem is permitted, the solutions occur as soon as insight into all the relations involved is reached. This view seemingly demands that little or no improvement appear until the moment of insight is established when a sharp inflection in the curve takes place.

Those who believe that learning is essentially the formation of bonds claim that the burden of proof is upon the configurationists. They assert that trial-and-error events are frequently and definitely observable in animal behavior but that insightful conduct is rare. Moreover, insight is very likely the outcome of a longer period of mental exploration, during which trial-and-error combinations will have

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had ample opportunity to be formed, tested, and discarded; overt behavior is merely delayed until the appropriate response has been chosen.

It was for the purpose of weighing anew the evidence in favor of these different positions that the present investigation was designed and undertaken.

Materials and Procedure

In planning the experiment, we assumed that it would be easier to discover the rôles of insight and trial-and-error in the higher thought processes than in simpler motor performances. Problems were, therefore, selected from the field of thought. They were all constructed so that errors would abound until the 'principle' of solution was discerned, after which no further mistakes would appear, *i.e.* 100% failure preceded the 'critical moment,' which was followed by 100% success. The character of the critical moment and its dependence upon earlier trials was the center of interest. Consequently all the test problems were given the *O*s individually so that both *O* and *E* would have ample opportunity for observation and report.

Test problems. Ten tests were devised and presented the *O*s. In addition to the specific instructions which were given with every test, the following general instructions were read to the *O*s, so as to insure the appropriate mental set, at the beginning of every experimental session.

"During this period a number of different tasks will be put before you. Each task appears in serial form; your problem is to find the principle underlying each series. These principles are of such a nature that as soon as you have discovered the right one for each set, the solution of the remaining problems is easy. Do your best and watch carefully what happens to you. Don't be afraid to express your thoughts as you work."

Test 1: (Point sequence). The test sheet (see Fig. 1) was divided into three parts: the first contained a number of black dots; the second a number of small circles; and the third a number of small equilateral triangles. The pattern of each part was apparently haphazard, but as a matter of fact it was carefully constructed so that some of the items would appear on straight lines. *E* placed this sheet before *O* and said:

"Note well everything I do. A general principle underlies the movements I make. As soon as you find out what this is, do something similar with the other two spot-groups."

E then pointed to a dot which was at one end of a straight line of dots and to the next to the last dot at the other end of the row—to "a" and "b" in Fig. 1. *O* was told to do the same with the circles and to give his reasons. If he falsely interpreted the instruction, a second illustration was given him by *E*'s pointing to "c" and "d." If the principle was still undiscovered, a third illustration was given by pointing to "e" and "f"; and so on until the solution was achieved or defeat acknowledged. In this and all of the other tests, *E* noted *O*'s activities during the problem solving and also at the end of each experiment recorded *O*'s verbal reports.

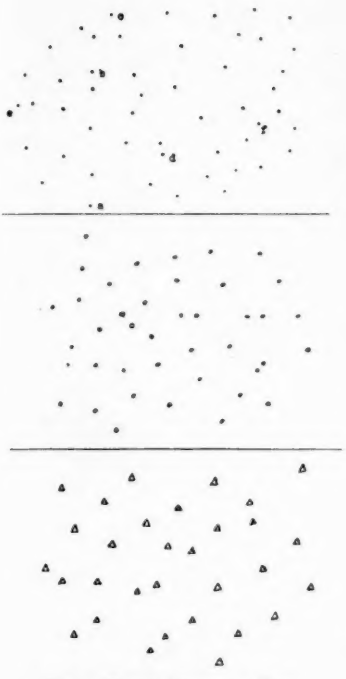

FIG. 1. POINT SEQUENCE TEST

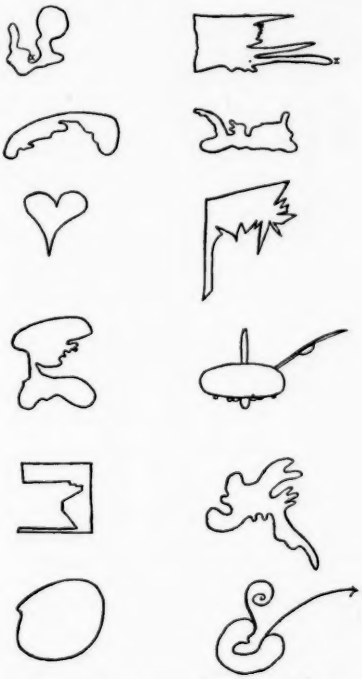

FIG. 2. CONCAVITY-CONVEXITY TEST

Test 2: (Concavity-convexity). A sheet containing a paired arrangement of rounded and angular figures (see Fig. 2) was placed before *O*. *E* then said:

"Watch me. I point here and there. There is a reason why I have done just what I did. Please do the same with the remaining figures."

As *E* spoke, he brought his pointer down at the inmost concave indentation of the left member of the pair and at the outmost convex projection of the right member —to the points marked "x" in Fig. 2. If *O* got the idea at once as evidenced by his ability to indicate the corresponding concavities and convexities in the other figures of the series, no further action occurred, but if he had followed a false clue, another illustration with the second pair was offered—and so on, until successful.

Test 3: (Sentences). A page containing the following sentences was laid before *O*.

(1) The youth loves the beautiful maiden.
(2) Sound is localized externally.
(3) Why are the students drawn to socialism?
(4) I developed the pictures myself.

(5) Solitary study of an experimental science can never be a substitute for observation.
(6) The aforesaid guaranty will be honored by all our shops.
(7) The committee wishes to favor all protestants.
(8) The inclosed bill is due upon presentation.
(9) He is the oldest user of the library.

As *O* turned to these sentences *E* remarked:

"Read carefully the first two sentences. The underlined words in both have the same relation to the whole sentence. Show me the corresponding terms in the other sentences."

When *O* made a wrong selection, *E* said, "Incorrect," when a correct selection *E* said, "Right." If no progress had been made by *O* in 5 min., *E* illustrated the problem in the next line; and so on, until the end of the list.

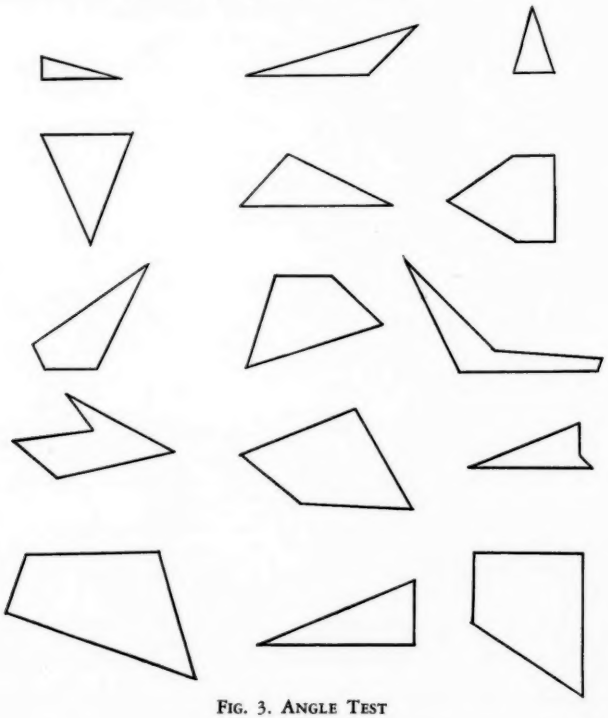

FIG. 3. ANGLE TEST

Test 4: (Angles). Each horizontal row of geometrical figures, shown in Fig. 3, was considered as a unit. *E* pointed to the most acute angle in the first figure, the most obtuse angle in the middle, and the most acute one again in the last; and then

requested *O* to repeat this pattern with the others. Examples were continued until *O* 'caught on.'

Test 5: (Inverted analogies). Test 5 was a deviation from the ordinary analogies test used in many measures of intelligence.

(1) steel : hard : : burden : light
(2) stomach : middle : : head : bottom
(3) one : two : : today : ————
(4) egg : oval : : sun : ————
(5) pot : cook : : broom : ————
(6) sky : blue : : snow : ————
(7) Socrates : wise : : Hercules : ————
(8) acids : sour : : honey : ————
(9) lamp : stove : : flame : ————
(10) eat : hungry : : sleep : ————
(11) feathers : light : : diamond : ————
(12) mouth : hand : : eat : ————

Instead of filling in the normally expected analogue, *O* was first required to find it and then give its opposite. The instructions read:

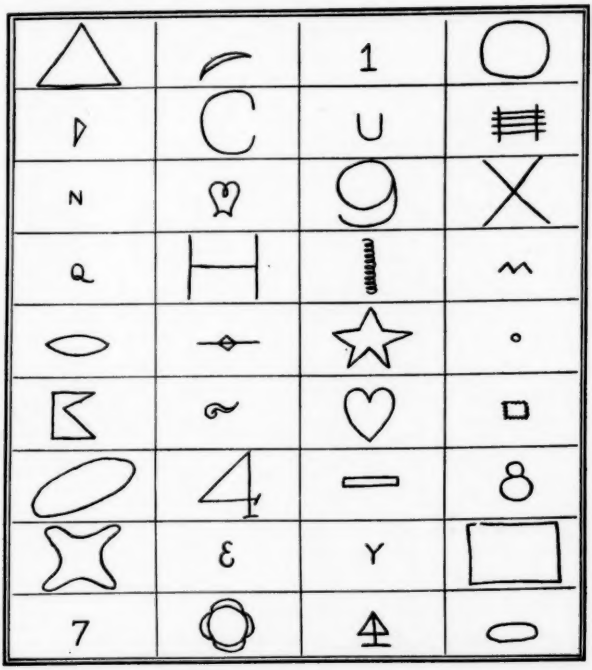

FIG. 4. FAMILIAR FIGURE TEST

"The underlined words in the first two model sentences are deemed correct because no other words satisfy the principle implicit here. Analyze the examples for the rule and apply to the remaining items."

If the 'point' was not soon seen, added helps were given *O* by cumulating, one by one, the right answers.

Test 6: (Figures). The material for Test 6 is shown in Fig. 4. Four columns of familiar outlines were placed upon a sheet, each row across constituting a unit problem, and consisting of one large circular figure, a large angular one, a small circular one, and a small angular one. The position each occupied, however, was purely haphazard. *E* pointed to the smallest rounded one in the first row and said, "I call this one correct." When necessary, the same act was performed with the second row, etc.

Test 7: (Syllogisms). A sheet containing the following was given *O*.

Please observe the following:

(1) All a is b;
Some a is c;
Some b is c. (True statement)

Why this is true can be demonstrated by means of circles, as in the sample figure. In similar fashion prove the truth or falsity of the following propositions.

(2) All Löso is Skandbeg;
All Skandbeg is Heifors;
All Heifors is Löso.

(3) Many beta are gamma;
All gamma are delta;
All beta are delta.

(4) Some x's are squares;
All squares are p's.
Some x's are p's.

(5) No Nirvana is Mahatma;
No Aurangzeb is Nirvana;
No Mahatma is Aurangzeb.

(6) Iron is not gold;
Whatever is not gold, has no value;
Iron has no value.

The first one—the model—was tested for its truth or falsity by means of Eulerian circles. It was hoped that *O*, in watching the rough sketch illustrating the device, would notice that the graphic representation of the degree of overlap (or freedom therefrom) was the essence of the method. He was then asked to test pictorially the validity of the remaining conclusions. If he failed to make headway, the second problem was illustrated, and so on, until learned.

Test 8: (Objects). This situation demanded the use of definite concrete material. A rather large collection of common articles was arranged upon the laboratory table in the following order:

(1) beech, oak, and pine pegs of uniform size.
(2) iron filings, face powder, and sawdust in similar dishes.
(3) lump of clay, glass disk, and porcelain plate.
(4) steel, lead, and brass bars of uniform length.
(5) leather strip, cloth strip, and absorbent cotton.

(6) rock candy, water, and bread.
(7) shaving brush, nail brush, and toothbrush.

E then said to *O:*

"Look at this first row of objects. I call this correct (pointing to the pine block). Now look at the next row. For the same reason I consider this correct (pointing to the face powder). Now try to identify the right item in the remaining rows."

If more help were needed *E* indicated the correct object in the next row, and so on until learned.

Test 9: (Prime numbers). A sheet containing the following was given *O.*

(A)	12 and 4	12 plus 4 makes 16 12 times 4 makes 48 12 minus 4 makes 8 12 divided by 4 makes 3. I consider 3 correct.
(B)	10 and 5	10 plus 5 makes 15 10 minus 5 makes 5 10 times 5 makes 50 10 divided by 5 makes 2. Either 5 or 2 is correct.
(C)	14 and 2	(16, 12, 28, 7)
(D)	20 and 1	(21, 19, 20, 20)
(E)	15 and 3	(18, 12, 45, 5)
(F)	24 and 12	(36, 12, 288, 2)
(G)	29 and 2	(31, 27, 58, 14½)
(H)	17 and 4	(21, 13, 68, 4¼)
(I)	110 and 10	(120, 100, 1100, 11)
(J)	39 and 4	(43, 35, 136, 9¾)
(K)	31 and 8	(39, 23, 248, 3⅞)
(L)	9 and 8	(17, 1, 72, 1⅛)

E then said to *O:*

"Notice the first pair of numbers at the top of this blank, 12 and 4. You understand, of course, that I can perform the four elementary arithmetical operations upon them such as: 12 plus 4 makes 16; 12 minus 4 makes 8; 12 times 4 makes 48; and 12 divided by 4 makes 3. I arbitrarily designate 3 correct, but I have adhered to a fixed rule in doing so. If I do the same with the next pair, 10 and 5, I consider either 5 or 2 as correct. Try to discover why and identify the 'right' number among the other possibilities."

Test 10: (Rational Learning). Peterson's original and unmodified form of the test was used with the accompanying instructions:[1]

"This is a memory-reason test. The letters A, B, C, D, E, F, G, H, I, and J are numbered in a random order from 1 to 10. I shall call out the letters in their order and you are to guess numbers for each letter till you get the correct number when I shall say 'Right.' Then I shall call out the next letter, and so on. This procedure shall be continued till you get each number right at the first guess twice in succession through the series, from A to J. Then you are through. You must ask no questions, but are to use all the mental powers at your command. You will be judged by the number of errors (or wrong guesses) you make and the number of repetitions from A to J required."

E's prepared sheet contained the following code with space for recording *O*'s responses:

Letters	A	B	C	D	E	F	G	H	I	J
Numbers	9	6	2	10	8	1	5	4	7	3

[1] J. Peterson, Experiments in rational learning, *Psychol. Rev.,* 25, 1918, 443-467.

It should be noted that this test deviates somewhat from the special thought-pattern which characterizes the nine preceding situations, but it was deliberately included to see if it would throw light upon differences in the methodology of solution, should such be found.

Results

A summary of the data, obtained in the tests described above, is given in Table I. The number of repetitions or separate illustrations required by the *O*s constitutes an index of learning which is reasonably free from an interpretative bias. Consequently, the results were first organized in this form.

TABLE I

Number Examples Required Before the Principle of Solution was Comprehended ("x" denotes failures)

O	Tests									
	1	2	3	4	5	6	7	8	9	10
A	x	x		x	6			7(?)	x	
B	1	4	x		1			4		
C	3	5	x		5			1(?)		
D		4	x	1	5	1	1		x	2
F		1	x	1	5			x	x	3
G	8			1		9			x	
H	3	1	x	1	3	4		4		
I							5			
J							6			
K							6			
L	x	6		1	3		4			
M	x	3			2		3			
N	1	3	x	3		2		3	x	
Av.	3.20	3.33	x	1.33	3.75	4.00	4.16	3.80	x	2.50

Total Average = 3.26

The different averages for each type of test and the total mean should be understood as having a low degree of precision, since the *O*s do not correspond in all situations and particularly since the values are derived only from instances in which solutions were actually obtained. If the x values had been carried far enough to procure definite figures, the means for all tests would undoubtedly have been higher. 'Instantaneous' solutions, *i.e.* those which occur after but a single illustration, are certainly not the rule. 'Sudden' solutions—those which appear after a longer interval of delay during which no apparent progress has been made—are often preceded by gross errors. Since the table does not reveal the qualitative features of the learning—upon which, in the last analysis, the theories really hinge—it will be necessary to examine in some detail the extensive protocols at this point. These excerpts show best the development of the thought processes toward the goal.

Test 1. Observer *A:* No solution was reached, even after 14 distinct operations had been presented. *A* thought of the following possibilities in order: (1) that parallel lines were being traced; (2) that two neighboring points must be in each series; (3) that a reversal of direction was the significant thing; and (4) that some geometrical figure was being formed by the movement. In common with other *O*'s loss of memory for the earlier models caused great confusion in the later stages, so that one may safely claim that if the solution is not reached within the first few trials, it is unlikely that it will be attained over a longer period.

B: Correct after one exhibition. *B* first saw a "cloud of spots" and before any action developed, he had adopted a critical attitude and asked himself, "What possible events can happen here?" The same instant that *E* pointed to definite spots, *B* saw the straight line relationship and hit simultaneously upon the principle—uniting the first and next-to-last member.

C: Correct after two models. There were no competing principles; the first suggestion which arose was the only meaningful one. "The idea was simply there. I simply saw what you demonstrated. No further problem was involved."

G: Correct at the eighth trial. *G* noticed the straight lines first, then the rhythm of movement; later, he tried incipiently, but rejected the notion of auditory tempo. After seven models, he observed that termination of the movement at the second point from the end was the only common element.

H: Correct on third demonstration. "The solution came immediately; until then my mind was an utter blank. Even when the idea was clear, I was not overconfident and thought the matter might be more complex." This record is interesting because it indicates that not all insight carries with it a conviction of its own correctness (cf. *A*'s results in Test 8).

L: Unsolved. "Either the task is meaningless or no task at all is involved. My guess is that you are just playing a joke on me to see how I would respond in the presence of utter nonsense. Frankly, I'm irritated."

Test 2. A: No solution was reached despite 30 min. of faithful work. Three distinct hypotheses appeared to observer *A:* (1) that similar angles were involved; (2) that projections of like shape were to be sought; and (3) that the correct point lay between two great bends. *A* explained that in this last he assumed a principle of equilibrium, and that if he was wrong, it was because he had been misled by the configurationist doctrine of 'good' figure or contour.

B: "At first I thought you wanted absolutely identical points, but then I assumed that only corresponding points were required. Consequently, I sought the farthest points inward in the left figure and the farthest point outward in the right one."

C: "The solution was purely accidental. I had no reason for believing it to be the correct one; I could just as well have taken some other. Subjectively, there was nothing novel about the manner in which the idea arose—merely a brief warming-up and then it was there!"

D: "Blind chance-taking was all that occurred. I should not have been surprised had my third answer been wrong. It was very much like a *guessing game* to me."

F: "The whole process was one of immediate perception. I saw at once that no conventional geometrical character was involved as the figures were too irregular

to be based on any curved or linear feature. The principle was quickly verbalized as 'the most indented first and then the most projected.' "

Test 3. B: " I tried definitely to locate some internal rational meaning to the sentences. My first thought was that words which were not absolutely necessary were to be underlined; next I tried the most important terms; then I assumed fixed parts of speech were involved. After that I was blocked, and although the possibility of counting letters occurred to me, it was rejected as too external a criterion." The last statement is of interest because it shows that the necessary 'lead' had been struck; without a guiding principle, however, to employ it, it proved abortive.

D: "From the beginning I considered a mechanical possibility and developed the following hypotheses in succession: words with double letters; first words of phrases; second word from beginning or end; number of letters in different words; plus at least a half-dozen other remote possibilities which have left me." There it should be noted that as the number of conceivable principles increases (or, as in this case, even approaches infinity) the chances of solution become progressively less.

Test 4. A: No solution. The separate suggestions were: (1) the absolute magnitudes were decisive; (2) all three angles constituting 180° together; (3) the angles opposite the largest sides.

D: "I wasn't sure of the correctness of my response when I gave it. The hypothesis was held tentatively until you [*E*] said 'yes'."

F: "The idea just came—how or why I don't know. It appeared as a single event. There was a feeling of indistinct looking followed by something arising. I see in the solution a very obvious characteristic of the figures." This report reveals the difficulties in describing a genuine experience of insight. Either the process occurs too rapidly or the essential antecedents cannot be found in focal consciousness; perhaps both facts are involved.

Test 5. A: The principle was finally discerned after much groping and by modifying partial insights into a more complete one. The notion of opposition was isolated after inspection of the original examples but it lacked precision; since *A* responded with "darkness" to the stimulus word "sun." Similarly, the term "lady" was given to match "broom," because that designation is the opposite of a servant," who would normally deal with the domestic items listed! Even after the correct word "weak" was given to "Hercules," the principle was uncertainly applied. Apparently—as every experienced teacher knows—it is possible to be on the right track and yet have one's vision of the goal obscured by some inner 'cloud.'

C: "The notion did not come with a leap—instead it was as though one were going in swimming and slowly sticking one's feet into the water!" Gradual growth characterized the formation of the concept in this case. The uncertainty with which the principle had been achieved was shown in the occasional errors of application after the solution had arrived.

D: "The correct idea arose after you said 'square' in connection with the word 'sun.' The right answer here was sharply pictorial and I saw the point vividly and instantaneously."

F: "After my initial errors, I intended to give some kind of paradoxical opposition but I offered the wrong attributes or antagonists. Finally, the exact re-

action appeared. Something happened within me resembling the electric light bulb which the comic papers place over the stupid fellow's head."

Test 6. G: "My most persistent error was the assumption that the number of sides was the crucial factor. The idea that size might be the criterion suddenly popped out; after a visual sweep there was added to this the principle of line quality. My numerous earlier errors were probably due to my adherence to absolute rather than relative judgments." This report is of interest in demonstrating that a solution often appears after an appropriate assembly of part or component ideas has been formed.

H: "My first hypothesis dealt with the matter of symmetry. This was quickly discarded. Finally, the complex principle came in sections: the features of bigness and littleness arose first, then roundness and squareness. Luck appears to play a big part in the searching process—one either sees the objective or not at all."

N: "Initial inspection showed that I could either proceed according to size or according to form. All at once it struck me that one could combine both criteria. Since this relation was easy to define, the rest was merely a matter of seeing whether it fitted the conditions."

Test 7. D: "This is a clever new wrinkle to me. It is plain that the conclusion is right or wrong only if the overlapping circles reflect the terms of the statements. Correctness or falsity is immediately seen by spatial position. After the first correct solution the thought process tends to be reduced to a visual reaction; the only difference being that a representative or symbolic character is presupposed."

J: "The correct procedure to be followed developed only slowly and gradually. Yet now the matter is completely clear. The isolated facts must first be placed in a larger total system; as soon as this coördination of relations has been arranged, the principle is plain."

Test 8. A: The solution in this case was ambiguous, hence the question-mark after the figure in the table. At first *A* asked, "Are all the objects symbols for shaving?" (for some reason, the shaving brush was a prominent item in his perceptual field). Then he sought for some common sensory object, such as size. At the very end, making a sudden grimace, he exclaimed, "How about *softness?*" But this idea, although correct, was rejected since the metal and wooden blocks seemed to lie beyond the range of application of this principle. One is surprised to find a solution actually at hand discarded because it does not meet a superficial test of fitness. The process of verification would seem to be an integral, although subsequent, part of the insightful process, for otherwise the insight remains imperfect (cf. *H*'s results in Test 1).

B: "Numerous principles suggested themselves—the tallest item, the one farthest to the right, the most frequently used, the dampest, etc. Then suddenly—after a survey of all the objects—came the idea of the least resistant."

H: "I classified the objects first into useful and useless items, and later tried the principle of hardness. I was 'stumped' by the metals, all evidently stout and shiny, until I reëxamined them and saw that one was lead. In trying to figure out the relation, I happened to dig my thumbnail into the lead block—and the whole thing cleared up at once."

Test 9. G: "My assumptions ran in this order: (1) the smallest number; (2) the odd number; (3) a combination of (1) and (2); (4) some geometrical series; (5) numbers which when represented graphically would form hexagons! The real answer is probably something wholly different, but the mathematical possibilities are too numerous for me to single out the right one."

Test 10. This test deserves separate consideration because its formal structure departs noticeably from the preceding ones. In this case, the *O*s sought for a principle although none other than purely arbitrary association was involved in the task presented. Nevertheless, it appears that rational, if not logical, procedures can be employed in deciphering a code, even though the code itself lacks a rational composition. The record of *D* illustrates this perfectly. He assumed at first that a reversed unit arrangement was being employed, *i.e.* that the initial letters of the alphabet were attached to the highest numbers and vice versa, but abandoned this principle as soon as the second letter failed to accord with it. To aid his memory for numbers already given, he bent down the first joint of the corresponding finger; consequently, he was able to suggest numbers only from those which clearly remained. If one did not work, he tried another. We have here a good instance of rational "tapping around" versus irrational "hopping around." It seems to the writer that the way in which the situation is attacked is itself evidence of insight into the necessary preliminary conditions. There is no reason for assuming that insight is restricted to occasions involving overt solution: it is perhaps just as much a matter of insight to control and regulate economically one's trial-and-error activities when one is convinced—in the absence of any definite set of meaningful relationships—that this is the only feasible way of reaching one's objective.

Discussion

An interpretation of the data secured in this study is difficult. The main result, however, may be phrased as follows: Learning can and does occur either by means of continual trial-and-error activity with an ultimate association of correct responses or through insight, *i.e.* through an immediate appreciation of the functions of various elements in a total situation. Certainly this is true on a *descriptive* level, even if the ultimate explanation of the fact of learning be sought in some *tertium quid* at present unknown.

One very obvious feature of the learning situations here employed is the frequency with which working hypotheses were developed and rejected before the desired insight made its appearance (see *G*'s record in Test 1, *A*'s in Test 5, and *B*'s in Test 8). In all of these cases, a number of possibilities was suggested and every one had to be tried before the correct principle of solution was discovered. The following question arises in this connection: Why do the numerous possibilities occur only in some tests and only with some *O*s? The answers obviously rest upon an analysis of the nature of intelligence. Lacking that we may hazard the opinion that success on the first trial may be merely the limiting case of a trial-and-error series (cf. *F*'s report in Test 4), in which a single unit of practice is adequate

to establish mastery. Formally, it would be comparable to the fixation of a conditioned response—as in many classic neurotic symptoms—on the basis of but one experience. Insights may then be considered as the outcomes of prior trial-and-error and occasionally emerge when that trial-and-error is at a minimum.

On the other hand, the assertion of many critics that 'insight' is merely the product of past experience, or a re-arrangement of habitual responses, is probably false, since the tasks—with the possible exception of the syllogism and rational learning tests—are all as novel as any which the *O*s are apt to encounter in their lives. The solutions are genuine ones and within the rather narrow range of complexity embraced by the problems are as original and creative as any inventive idea. They are inductions or inferences from specific material and always represent an act of achievement.

Phenomenologically, our descriptions of the moment of insight tend to be very meager (see *F*'s report in Test 4). It seems that in this respect it is very much like the 'determining tendency' of the Würzburg School in being effective in behavior but unanalyzed as a conscious state. In some manner or other, our mental gears are shifted—or, to vary the figure, the kaleidoscopic particles are redistributed—but the product and not the process is alone accessible to us. The apparent suddenness with which insight occurs, at least in some cases, suggests an analogy if not a neurological kinship to the perceptual facts of figure and ground, especially in the quick alternation obtained with ambiguous outlines. Much of the language of reasoning is derived from the terms employed in perception and this relationship is more likely generic than fortuitous. Hunting the face in the tree is usually preceded by many false identifications, just as the typical principle in this study was only revealed after three or more (cf. the average values in Table 1) plausible principles had been tried. Even 'false' insights occur, *i.e.* they arise suddenly and carry emotional conviction as intense as the 'real' ones until their inadequacy is established by further testing.

Further, it may be noted that there are degrees if not kinds of insight which should be distinguished. The terminological difficulties inherent in the concept have been discussed by the writer in another place.[2] If the data suggested by Test 10 be of general validity, there appear to be two types of insight—one involving a comprehension of the internal structure or organization of a situation, and the other involving sagacity in selecting appropriate methodological approaches to the task of problem-solving. Normally the two probably operate together as a unit but where the first part is im-

[2] G. W. Hartmann, The concept and criteria of insight, *Psychol. Rev.*, 38, 1931, 242-253.

possible because of the nature of the material the second may still exist.

On the animal plane, one probably also encounters two levels of insight: a broad one, characterized by an appreciation of the instrumental function of definite physical objects, and a narrow one, in which the understanding is limited to an awareness that in order to attain a particular end one must join one activity to another,—an *'Und'-verbindung* of the purest sort! A child knows that if one presses a button light appears, but only one familiar with elementary electricity realizes that this consequence is dependent upon the contact of two metal conductors, a charged wire, and the chemical nature of the filament.

The varieties of insight, as classified by Alpert in a study made with children,[3] were also found in our adult observers: (1) solution with *immediate* insight; (2) solution with *gradual* insight (see *C*'s results in Test 5); (a) *Partial* insight (see *F*'s results in Test 5); (b) *Complete* insight; and (3) solution with *sudden* insight (see *H*'s results in Test 8). In the case of immediate insight all the elements required by the complete configuration are present simultaneously; where gradual insight appears they are built up by accretion; and in sudden insight the imperfect configuration is quickly closed when a crucial element fits into its proper place, *i.e.* a sharp transition occurs as soon as some significant or useful fact has been observed. This last, of course, is the most representative and clearest case of insightful behavior, being preceded by a long or short period devoid of success. Between the solution of the problem and the conviction that it has been correctly solved falls an interval of varying length; but regardless of this delay the solution itself follows once the prerequisite conditions have been fulfilled. Too much stress should not be laid upon this factor of 'suddenness'—a relative term at best—since we are dealing with an inconvertible proposition, *i.e.* all sudden solutions are insightful but not all insightful solutions are sudden!

The typical hyperbolic learning curve differs markedly from any graphic records of progress which one could construct for the instances above. Nevertheless, this fact alone—of which much has been made—does not constitute proof that two utterly different species of learning are involved. The writer suspects that the negatively accelerated curves which one obtains from relatively simple motor performances differ as much from each other as they do from curves portraying gradual insight. The difficulty probably lies in the unit of measurement. In the case of insight, minute advances do not manifest themselves externally as soon as made, *i.e.* every added unit of time does not result in an increase in the amount done, but are first accumulated

[3] A. Alpert, *The Solving of Problem-Situations by Pre-School Children*, 1928, 41.

into higher units before overt behavior occurs.[4] A 'principle' is of the order of a higher unit and simply cannot function except in its entirety: it has a definite 'all-or-none' character. But this certainly does not imply that the trial-and-error activity which precedes the emergence of the insight is irrelevant to its formation.

It is curious to see how the classic problem of the transfer of training is re-opened, or at least re-interpreted, by the concept of insight as used by the defenders of the *Gestalt* doctrine. Once the insight into a certain kind of situation has been acquired, similar situations different in content but alike in form (configuration) can be adequately adjusted to without further training. Transposition of "structure-functions" unquestionably occurs in lower animals: why then should we hold to an exaggerated specificity of learning in man? To be sure, transfer takes place only within definite limits for if the later configuration be too foreign to the earlier a new insight will be demanded. There are limits to the generality which insights can assume; *e.g.* an animal which has mastered one "Umweg" problem is not thereby qualified to solve all such situations! The configurations presented by new tasks must not deviate too widely from the original insight if the latter is to be successful. Just because an adult knows how to solve the equation X^2 plus $MX = N$ by adding $(M/2)^2$ to both sides does not of itself enable him to determine the weight of a body through indirect balancing of the scales or to equalize the resistance in a Wheatstone bridge—even though they all share in common the property of maintaining equilibrium by adding certain quantities to both sides.

Conclusions

Trial-and-error activity when applied to problems which are capable of yielding general principles eventuates in insight as understood by the *Gestalt* psychologists. The required act is firmly learned as soon as, and only when, the insight has been achieved. Depending upon unknown conditions in the learner's organism, the insight may make its appearance immediately, gradually, or suddenly after a delay. Once acquired, the insight may be uncertainly applied to concrete situations or even forgotten, although it is normally very stable. The insight itself seems to be a configuration made up of familiar parts operating in a new setting. Trial-and-error experimentation appears to be necessary in inserting and rejecting the elements of a series of tentative organizations before the right one, *i.e.* the one which realizes the goal-satisfaction, is completed. Whether the same state of affairs holds for other types of problems must be determined by later research.

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[4] Cf. E. L. Thorndike, *Educational Psychology*, 2, 1913, 338-349.

CURVES OF MEMORIZATION AFTER DIFFERENT AMOUNTS OF PRACTICE

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It has long been known that practice upon successive samples of a given learning material is normally accompanied by an increase in the speed of learning as measured in trials, time, or errors. In his study of the form of the curve of memorization Kjerstad obtained this result and added to it the very important conclusion that as the number of trials taken to learn successive samples decreases the form of the curve of memorization remains essentially unchanged.[1] He based this upon the finding, yielded by analysis of Vincent curves, that the percentages learned in halves, thirds or sixths of the total trials did not vary importantly between the two halves of the total number of lists learned. This general conclusion has recently been partially confirmed by White,[2] who has shown that the percentages of 12-syllable lists correctly anticipated at successive quarters of the total learning of the lists in positions 1, 7, 13, 19, and 25 of a practice series and in the corresponding positions of a succeeding continuous work series are substantially the same with the single exception of those of the list in position 1 of the practice series. The latter has lower values at the 25- and 50-percentile points than do any of the others. The importance of this generalization that the percentages learned in given fractions of the total learning period remain a constant through the diversities present in the repeated learning of similar lists is great. The present paper reports a test of this generalization with the data of two different experiments, together with a study of certain other aspects of the curve of memorization.

EXPERIMENT I

Method. The first data to be discussed are those of an experiment on the variation in retroactive inhibition with varying time-interval.[3] Six groups of 20 college students each learned, by the anticipation method, lists of 10 two-syllable adjectives exposed at a 2-sec. rate on an electrically driven drum. Each list included an eleventh adjective which served as a cue and which was not learned. Learning was to a criterion of 3 successive perfect trials. A complete record was

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[1] C. L. Kjerstad, The form of the learning curves for memory, *Psychol. Monog.*, 26, 1919, (no. 116), 1-89.

[2] G. C. White, The form of the curve of memorizing, *J. Exper. Psychol.*, 15, 1932, 184-194.

[3] This experiment was assisted by a Grant-in-Aid from the National Research Council.

taken of the correct anticipations or of the failures to anticipate at each position and during each trial.[4] A quantitative picture of the course of learning from the first trial to the criterion is thus provided.

At the beginning of the experiment, immediately following careful verbal instructions, every *S* learned two lists at a single sitting. A 5-min. interval elapsed between the completion of the first and the beginning of the second. The learning of these two lists constituted a practice sitting for the *S*s, all of whom were hitherto entirely unacquainted with learning by the anticipation method. In the main experiment each of the 6 groups learned under either a rest or a work condition and each learned a total of 5 lists to the criterion and 5 other lists to 10 repetitions each. Under the work conditions the 10 repetitions constituted interpolated learning while in the rest condition they were taken after the original list had been relearned in order to keep the amount of practice the same under both rest and work conditions. Groups A, B, and C[5] learned under the rest condition, and Groups D, E, and F under the work condition. In the intervals between learning and relearning the groups working under the rest condition did no more learning of this kind; Groups D and E learned the interpolated list immediately following the completion of the original list to the criterion; and Group F had the interpolated list just prior to the relearning of the original. In all conditions the 5 intervals used were 20 min., 1, 24, 48, and 144 hr. The total period covered by the experiment was thus about 2 weeks.

The 5 conditions were gone through in an order which counterbalanced the factors of both practice and list for the conditions. When the conditions were ordered in serial position from first to fifth, regardless of interval, the lists were so arranged that half of the *S*s took them in one order and the other half reversed the order of each successive pair. Under the practice conditions all *S*s learned the same lists in the same order. All of the lists used are of homogeneous material and are virtually equivalent in difficulty and there is small reason to infer that the factor of the particular list has had any disturbing influence upon the conclusions.

One *E* was in charge of Groups A and E, another was in charge of Groups C and D, while the two other groups each had a different *E*. The 4 *E*s, in addition to a training course in experimental psychology, had been given careful training by the writer in the use of the anticipation method and in the precautions necessary to the experimental conditions employed, and their work was under careful supervision throughout. Conditions of lighting and of the general environment were kept constant and vigorous effort was made to maintain as rigid a constancy of conditions as is possible when human beings are the experimental *S*s.

Treatment of data. In combining his individual curves Kjerstad employed the Vincent technique[6] whereby amounts learned in successive equal fractions of the

[4] These records were taken on special forms devised by Dr. Arthur W. Melton of Yale University and printed by the Printing Office of the Yale Press.

[5] Group C, working under the rest condition, did not learn the five balance or post-relearning lists to 10 repetitions each, but otherwise the conditions were exactly the same as for the two other groups working under these conditions. The Vincent Curve values show no sign of having been affected by this difference and Group C has, therefore, been included without separate treatment.

[6] S. B. Vincent, The function of the vibrissae in the behavior of the white rat, *Behav. Monog.*, 1, 1912, 1-81.

total learning time are combined, and the same method has been used with the present data. The memorization curves for every *S* and for every list were first drawn by plotting trials to the criterion of 3 perfect in succession on the abscissa and number of correct anticipations on the ordinate. The data for every curve were plotted on a proportional scale such that each had an abscissa 200 mm. in length. This was aided by tables showing the exact divisions of the abscissa for each number of trials from 10 to 100.[7] When all curves are plotted on abscissas of equal length, it is a simple matter to divide them into any desired fraction. The values for equal fractions of the total number of trials may then be added and a composite Vincent curve drawn from the pooled data.

This method reduces to a common base a large amount of highly variable data and makes possible a general interpretation which would be impossible if one dealt only with the individual curves. It has the disadvantage that it masks the individual characteristics of the data and provides, in the composite curve, a result some distance removed from the raw individual phenomena. All measures of central tendency do this, however, and the Vincent procedure yields a distortion which is no greater than that of any measure of central tendency. If, moreover, one is always careful to compare the features of the composite curve with those of the individual curves, there is slight danger of drawing conclusions which are only artifacts of averaging.

Results. Any examination of Kjerstad's conclusion that the form of the curve of memorization remains constant as the number of trials taken to learn decreases must first make certain that the decrease in trials has actually occurred in the data to be examined. More complete data upon the changes accompanying practice at successive lists of this material under the three work conditions and one rest condition (Group A) are to be published separately. A brief summary of the relevant facts will suffice for the present purpose. The mean number of trials taken by the 80 *S*s of Groups A, D, E, and F to learn the first practice list to a criterion of 3 perfect trials is 35.4, while the mean number required to learn the last of the 7 major lists is 11.5. This is a percentage decrease of 67.5, and the corresponding percentages for Group B and C are 59 and 62. These decreases in the numbers of trials required to learn are clearly large enough to give an opportunity for changes in the form of the curve of memorization to appear if such changes are correlated with the effects of practice. The data make possible, therefore, a test of Kjerstad's conclusion.

Kjerstad divided his curves into halves, thirds, and sixths and failed to find with any of these divisions any uniform suggestion of a change of form with practice. The curves yielded by the present experiment will be presented first in halves, for the purpose of direct comparison with Kjerstad's. The

[7] I am greatly indebted to Dr. Arthur W. Melton of Yale University who prepared these tables and who very kindly provided me with copies.

percentages learned by each group in the first half of the learning period are shown for the first practice list and for the final list, together with sigmas of the means, in Table I. Only the data for the first halves are given, since they give an adequate picture of the facts and since the data for the second halves can, if desired, be obtained subtractively.

TABLE I

PERCENTAGES LEARNED IN THE FIRST HALVES OF THE LEARNING PERIOD

Lists	Rest Groups			Work Groups		
	A	B	C	D	E	F
Practice	54.7±4.9	48.7±4.3	60.9±4.5	59.2±5.3	64.8±4.2	62.7±3.2
Final	77.0±2.4	76.5±2.3	75.2±3.1	72.8±4.5	75.4±3.7	78.3±2.8

The 6 groups show without exception a clear increase with practice in the amount learned during the first half of the learning period, and, hence, a definite shift toward a more pronounced negative acceleration of the curve of memorization. These data fail, therefore, to verify Kjerstad's conclusion that the curve is a constant in form regardless of the stage of practice. The average change for the 6 groups amounts to 17.3%, which although small in comparison with the 59% to 67% decrease in trials required to learn, represents a considerable change in the form of the curve. In view of the consistency of the data, it is scarcely necessary to present the sigmas of the differences in order to validate the conclusion. These sigmas may readily be found, if desired, from the data given. Not all differences are three times sigma (those for Groups C, D, and E are not), but the chances are 97 in 100 that the least reliable difference (Group E) is greater than zero, and this together with the complete consistency of the 6 groups permits confident acceptance of the conclusion that practice affects the form of the curve.

It will be noticed that the percentages of the practice list learned in the first half of the period vary considerably among the 6 groups and that the corresponding percentages of the final list vary much less. Moreover, the sigmas of the means of the final lists are considerably smaller. A second effect of practice is, thus, to render the Vincent curve-values of the individual groups less variable and the several means more nearly the same in magnitude.

This analysis of the curves in terms of halves is at best a rough procedure and can give only a general picture of the actual situation. Divisions into thirds would be only slightly better and a finer analysis than even sixths can provide is desirable. In order to arrive in greater detail at the influence of practice upon the form of the curve, the individual curves have been divided into tenths and a composite curve drawn for each group to show the

cumulative percentages of the total list learned in successive tenths of the total learning period. The curves of the 120 *Ss* have then been pooled to give a single curve at each of the 10 points in the course of practice; and the data for such a composite curve are presented in Table II. To present the data of the individual groups would necessitate cumbersome tables and would add little to the briefer presentation of Table II which gives, in addition to the mean values of the practice and the final lists, the mean deviation of the 6 groups from the mean of all. A comparison of the two columns of means shows that the final list has at every possible tenth a higher mean than has the practice list. The differences are largest during the first half of the learning period and decline thereafter. All save that at Position 9 are statistically reliable. The curves of the 6 separate groups agree almost perfectly with these characteristics of the composite curve. At only 3 points out of 54 is a value for the final list lower than the corresponding one for the

TABLE II

VINCENT CURVE-VALUES FOR FIRST PRACTICE AND FINAL LISTS
(N is 120 at each tenth)

Tenth	Mean	σM	V	Group A.D. (6 Groups)	Mean	σM	V	Group A.D. (6 Groups)
1	17.0	1.15	74.4	2.8	30.6	1.47	52.6	4.6
2	32.0	1.57	53.7	4.6	47.3	1.43	32.9	5.6
3	41.9	1.86	48.4	8.1	59.7	1.74	31.8	3.9
4	54.0	1.80	36.4	4.2	68.1	1.54	22.1	3.0
5	58.3	1.89	32.4	4.5	75.9	1.43	18.5	1.4
6	67.9	1.56	25.0	2.7	78.6	1.45	20.1	2.0
7	72.6	1.57	23.6	2.1	83.0	1.21	19.0	1.9
8	74.9	1.78	25.7	3.5	83.6	1.28	16.7	2.7
9	81.2	1.47	19.8	3.6	83.6	1.20	15.6	2.2
10	100.0	0	0	0	100.0	0	0	0

practice list and then by an insignificant amount and at the 8th or 9th division of the curve. All groups agree in showing the largest differences during the first half of the learning period. It must be concluded that the amount of practice which has intervened between the first and the last lists has definitely increased the initial spurt and sharpness of the negative acceleration of the memorization curve and has uniformly affected the magnitude of the values at each succeeding tenth. The influence of practice is greatest upon the first half of the curve, *i.e.* upon the amount learned during the first half of the learning period.

The sigmas of the means are, even in the early part of the learning period, relatively small and these mean values may be considered to have a high degree of stability. The relative variability of each list at each tenth decreases almost uniformly from a high point at the initial tenth to a low one at the last tenth, the 9th, at which variability is possible. At every point the

variability of the final list is less than that of the first practice list. Practice has uniformly and in considerable amounts decreased the variability of the Vincent curve-values.

The largest single decrease in the number of trials required for mastery occurs from the first to the second practice list and it is necessary to inquire whether there has been a comparable change in the values of the curve. These are, at successive tenths of the learning of Practice List B: 21.0; 36.6; 52.8; 63.4; 70.5; 75.6; 78.9; 84.3; 82.3; and 100.0. A comparison of these with the figures of Table II will show that the curve for List B is higher at each tenth than is that for List A and that the gains made by the former are greatest over tenths 3 to 8. The curve for the final list is higher, except at the 8th tenth, than is the curve for List B. The shift with practice is, thus, relatively rapid at first but extends beyond the initial practice list.

The relative variability of the means of the 6 groups from the mean of all is nowhere large and decreases irregularly as learning proceeds. Computation of the relative inter-group variability (100 A.D./M) would show that at every point the variability of the final list is smaller than that of the practice list and that at many points the difference is considerable. The conclusion reached from examination of the percentages learned in the first halves of the learning period that the effect of practice is to reduce the inter-group variability of mean Vincent curve-values or to render the mean values of different samplings more stable is, therefore, reënforced.

The problem of the form of the curves of acquisition during the very early stages of the process has been for some time both active and unsettled,[8] and it will be significant to inquire what form these curves have near the point of origin. Division into tenths has yielded a composite curve with a general negative acceleration. In the case of the practice lists, however, which require a mean of about 35 trials for mastery, each tenth includes 3.5 trials, on the average, and thus the form of the curve in the first few trials is obscured. The initial form may be seen more clearly if a composite is constructed from more minute divisions of each individual curve. Such a composite curve has been drawn in fortieths. Inasmuch as we are primarily concerned here with the early portion of the curve and as the general form of its later portion can be seen from Table II, the composite will be reproduced for the first ten fortieths only (Fig. 1). The early part of the curve of the practice list is virtually linear, while the corresponding portion of the final list has an early initial spurt and subsequent negative acceleration. The

[8] Cf. E. Culler, Nature of the learning curve, *Psychol. Bull.*, 25, 1928, 143-144.

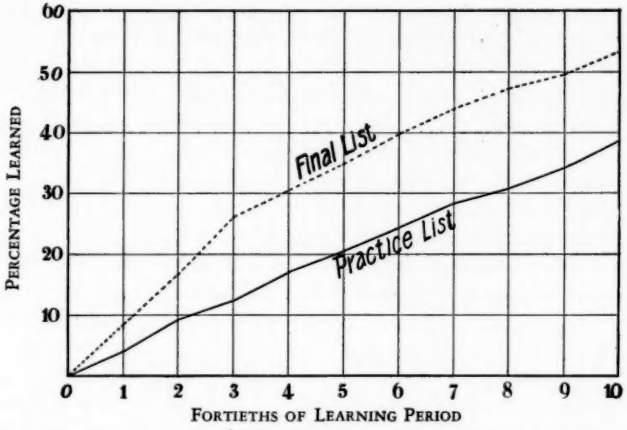

FIG. 1. VINCENT CURVES FOR THE FIRST 10 FORTIETHS OF THE TOTAL LEARNING PERIOD
(Adjectives)

conclusions already drawn regarding the influence of practice upon the form of the curve are reënforced by the data of this figure.

At this point another feature of Table II and of the curves of its constituent groups should be mentioned. Both practice and final lists show in every group the clear end-spurt which Peterson[9] and White have reported.[10] The magnitude of the spurt is only slightly reduced by practice.

EXPERIMENT II

Method. In this experiment 12 college students, none of whom was familiar with either the anticipation method or with any experimental work on memorizing, learned lists of 8 syllables from the Glaze 0% group[11] under the same conditions as those employed in Experiment I. On each of 3 practice days one list was learned to a criterion of 3 successive perfect trials. This was followed by the main experiment in which they learned, either partially or completely, under conditions described elsewhere,[12] 22 lists of syllables of varying associative value. On the day following the completion of this experiment, they learned another list of 0% associative value. The order in which these 0% lists were learned was

[9] G. M. Peterson, Negative acceleration with material of varying difficulty, *J. Exper. Psychol.*, 11, 1928, 40-44.

[10] White, *op. cit.*

[11] J. A. Glaze, The association value of non-sense syllables, *Ped. Sem.*, 35, 1928, 255-267.

[12] J. A. McGeoch, The influence of degree of interpolated learning upon retroactive inhibition, this JOURNAL, 44, 1932, 695-708.

counterbalanced to give all lists equal average position. The data have been treated in the same way as were those of Experiment I.

Results. The percentages learned in the first half of the learning period at the four positions in order are: 55.3; 60.0; 66.5; and 67.9. This regular increase with practice supports the conclusions of Experiment I.[13] This increase is seen more clearly, however, when the composite curves are drawn to represent the amounts learned at successive tenths of the learning period. The numerical values of these curves (Table III) show a clear tendency to increase in magnitude, especially in the first half of the learning period, during the three practice days. There is, also, a discernible but less regular tendency for the values in the second half of the period to increase. After the intervening practice at 22 mixed lists, the amount learned in the initial tenth is larger, as is that learned by the end of every tenth except the second. The large amount of practice has been followed by no great changes in the form of the curve, but there is an unmistakable shift toward a larger initial spurt with the resulting augmentation of the later portion of the curve.[14]

TABLE III

CUMULATIVE PERCENTAGES OF PRACTICE AND FINAL LISTS LEARNED IN SUCCESSIVE TENTHS OF THE TOTAL LEARNING PERIOD

Tenth	Position of List			
	Practice			Final
	1	2	3	1
1	13.6	15.5	25.6	30.7
2	29.6	39.6	44.3	43.2
3	41.2	35.6	51.0	53.6
4	43.7	52.1	53.6	66.0
5	55.3	60.0	65.2	67.8
6	75.6	70.0	70.2	77.2
7	69.8	64.0	74.0	85.0
8	72.5	78.3	80.1	81.2
9	83.6	79.3	86.5	87.0
10	100.0	100.0	100.0	100.0

[13] The mean numbers of trials required to learn these lists to a criterion of three perfect trials in succession reduced from 60.8 at the first practice position to 22.4 at the position immediately following the main experiment. This large reduction gives ample opportunity for changes in the curve of memorization to occur if practice is accompanied by such changes.

[14] To apply statistical criteria to the means and differences of this experiment is, in view of the small number of *S*s, scarcely fruitful. The uniformity of the shift in the Vincent curve-values is, however, sufficient guarantee of the trustworthiness of the general tendency. On the face of the results there is an inversion at the 6th tenth in the first two practice lists, none in the third, and one at the 7th tenth in the final list. Whether a plateau phenomenon, which shifts toward the end of the list or disappears entirely with practice, is indicated is hard to say with any certainty.

It should be noticed that in all of these curves the end-spurt phenomenon clearly appears, and that this end-spurt is slightly reduced by practice.

A comparison of the values of the two experiments shows that they are, in the practice position, roughly the same at corresponding tenths and that, likewise, there is a rough equivalence in the final list. If anything, the adjectives show a slightly greater initial spurt than do the syllables and, with approximately half the amount of practice in terms of lists learned, as much gain.

The form of the curve in the early part of the total learning period has been examined by the method employed in Experiment I, and Fig. 2 shows the first 10 fortieths of the Vincent curves for the first practice and the final list. The curve for the former has a slight initial positive acceleration and would, if completely drawn, be S-shaped. Judged by the number of trials required to learn, this list of syllables was much more difficult than was the first list of adjectives in the preceding experiment and the greater tendency toward S-ness under these conditions makes it appear that, with naïve *S*s and with increasing difficulty of material, the curve may be expected definitely to assume this form.[15] Practice, however, produces a change to an initial spurt and negative acceleration, as the curve for the final list demonstrates.

Discussion

Both experiments have shown a change with practice in the amounts learned at the end of successive tenths of the total learning period and that, particularly, increasingly large percentages are learned in the first few tenths of the period as practice continues. This differs sharply from Kjerstad's conclusion that the percentages learned in corresponding halves, thirds, and sixths of the learning period are unaffected by practice. It is, however, questionable whether the present experiments should be construed as contradicting Kjerstad's in any fundamental sense, on account of certain differences in experimental method and in treatment of data.

There are several marked differences between the materials and procedures of Kjerstad's study and those of the present experiments. With one exception he employed paired associates with a test in writing after each exposure and a 3-min. intermission between the test and the succeeding exposure. The single exception consisted of serial nonsense syllables, but these were presented at a 3-sec. rate and a written reproduction was attempted after each complete presentation. The differences which are obvious be-

[15] Cf. the conclusions reached by Culler, *op. cit.*

tween the methods of the two experiments have doubtless been responsible for some of the differences in the results. Certainly the longer exposure interval and the written reproduction would operate to hasten the attainment of a practice level, but in the absence of specific knowledge regarding the character of this influence it is hardly worth while to do more than call attention to its presence. It should be noted, also, that for the purposes of any precise analysis of the curve Kjerstad's methods of measuring progress are rather rough. About half of his *Ss*, moreover, were graduate students in psychology whose knowledge of the subject, training in experimental work, and probable experience at memorizing may have brought them to a somewhat advanced stage of practice before the beginning of the experiment.

An important consideration in accounting for the apparently contradictory results lies in the number of lists represented in Kjerstad's comparisons. He does not compare the curves obtained at the beginning of practice with those drawn at some later stage. His relatively few *Ss* each learned large numbers of lists of each material and in studying the effect of practice upon Vincent curve-values he compared the average percentages reproduced at halves, thirds, or sixths of the total period during the learning of the first half of the lists learned with those reproduced correspondingly during the learning of the second half of the lists. This comparison of the pooled values of fairly large numbers of lists necessarily obscures any change which

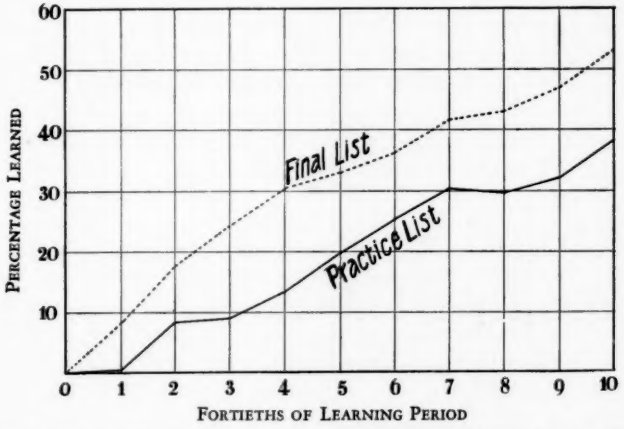

FIG. 2. VINCENT CURVES FOR THE FIRST 10 FORTIETHS OF THE TOTAL LEARNING PERIOD
(Nonsense syllables)

may have occurred during the learning of the first few lists since these enter into the composite which includes more than the first two or three lists. The present results make it seem probable that practice effects diminish rapidly with continued learning and it is a reasonable inference that his averaged results masked these early changes. In this paper the results which have been pooled were all from an equivalent stage of practice and comparisons between the first list learned and one learned after intervening practice are designed to reveal changes which Kjerstad's method would conceal. Shifts as large as those reported here, however, had they occurred in his results, should have operated to reduce somewhat the means for the first halves of his lists. That such a reduction is not visible may mean that the factor under discussion is less potent to account for differences than has been suggested or it may be due to differences in experimental method and in *Ss*. It is at least a reasonable hypothesis that the latter is the case.

The present results are also, on the surface, at variance with those of White, but the contradiction is more apparent than real. His results show smaller percentages during the first half of the period of the first list learned in the practice series. The next sample taken is from the 7th position and here the values at the 25- and 50-percentile points are higher. Samplings after further practice show no additional gains. These results do not mean that there is no shift in Vincent curve-points with practice, but rather that the shift is over by the time the 7th list has been reached, at least in so far as an analysis by quartiles will reveal it. This finding that the curve is unchanged by continued practice after the first 7 lists accounts for the fact that the final list in Experiment II has values which are only slightly larger than those of the third practice list.

The question at once arises whether the results of the present experiment imply any modification in the interpretation of Kjerstad's other conclusions. It will be best to confine discussion of this question to the single case of the influence of the length of the list upon the form of the curve of memorization, since Kjerstad's conclusion that the latter is independent of the former has been confirmed by Robinson and Heron[16] and by Robinson and Darrow.[17] The lists employed in the present experiments have been confined in length to 10 adjectives and 8 nonsense syllables. The percentages learned in successive tenths of the learning period are, however, much the same for the two at the initial and final stages of practice, although the values for the

[16] E. S. Robinson and W. T. Heron, Result of variations in length of memorized material, *J. Exper. Psychol.*, 5. 1922, 428-448.

[17] E. S. Robinson and C. W. Darrow, Effect of length of list upon memory for numbers, this JOURNAL, 35, 1924, 235-243.

second and third practice lists deviate somewhat from these percentages. While no direct conclusions can be drawn from these small differences in length of list regarding the influence of this factor, there is nothing in the results to indicate that the generalization stated above is in any way contradicted. It is, however, a probable inference that one modifying clause should be added to the effect that the generalization holds within or at any given stage of practice. The addition of this clause assumes that the curves for lists of different lengths would show comparable changes with practice, but this assumption seems readily justifiable. The Kjerstad-Robinson law, as this generalization may be called, would then read: the form of the Vincent curve of memorization at any given stage or condition of practice is independent of the length of the list.[18]

It is worth emphasizing that the 6 equal samplings of Experiment I yield results which show a relatively high degree of uniformity. This finding of relative constancy in results from small populations is one effective answer to the criticism that conclusions from laboratory work done with small numbers of *S*s cannot be generalized. No one, presumably, has supposed that such results could be generalized for all possible *S*s, but only that they were valid for all *S*s of the same kind and under the same conditions, an assumption which is strongly supported by these data.

Conclusions

(1) Practice at memorizing lists of adjectives and of nonsense syllables by the anticipation method and under the other conditions of these experiments is consistently accompanied by changes in the percentages learned in successive fractions of the total learning period. The more important of these changes are stated in the following generalizations. (a) Practice increases by considerable amounts the percentages learned in the first half of the total learning period and particularly in the early tenths. The slope and extent of the initial spurt and the sharpness of the subsequent inflection are, thus, increased. The curve for nonsense syllables is shifted with practice from a slight initial positive acceleration to an initial spurt and succeeding negative acceleration. (b) Practice also decreases the variability of the percentages learned in successive fractions of the total period and renders more uniform the means of different samplings of *S*s.

[18] In the experiments by Kjerstad, Robinson, Heron and Darrow the factor of length of list was studied with pooled data in which all stages of practice were averaged and in which, therefore, there was no opportunity for the effect of practice to become apparent. Under these conditions the law is a generalized one for a composite of all stages of practice involved in the experiment.

(2) It is suggested, in the light of these results, that a clause should be added to the Kjerstad-Robinson law which would modify it to read thus: *the form of the Vincent curve of memorization at any given stage or condition of practice is independent of the length of the list.*

(3) The curve of memorization for lists of nonsense syllables of 0% associative value shows, at the first practice position, an initial positive acceleration and the total curve is S-shaped. With practice, as has already been stated, early negative acceleration supervenes, but the final spurt phenomenon, or final positive acceleration remains. The corresponding curve for adjectives has an early linearity. These facts support the conclusion that with unpracticed *S*s and difficult materials the curve of memorization will tend to be S-shaped.

A PSYCHOPHYSICAL STUDY OF VISUAL GESTALTEN

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There has been relatively little psychophysical work done from the point of view of Gestalt psychology. The configurationists approach the problems of relational judgments quite differently from the psychophysicians.[1] The application of the methods of psychophysics to the problems of Gestalt may prove difficult, but there appears to be no *a priori* reason why an attempt should not be made.

The present paper marks an attempt to apply the constant method to one of the outstanding problems of Gestalt psychology, *i.e.* to the study of visual *Gestalten*.

The particular problem that we have undertaken is to discover whether a change in the configurational relationship would noticeably affect the thresholds for the judgment of the separation of two visually presented bars. In one case the judgment was to be made within a spontaneous dominant configuration and in the other it was to be made between two such spontaneous dominant configurations which apparently were in opposition to the particular configuration required for the judgment. The configurational membership was determined by the color of the bars, red and blue being used in two arrangements.

METHOD

Apparatus. Fourteen comparison-cards and one standard card were successively presented by means of a Whipple Disk Tachistoscope for an exposure-time of 75σ. This duration was adequate for a clear impression but too brief for exploratory eye movements.[2] Except for the first few hours the complete experiment was conducted in a dark room.

The 14 Comparison-Cards were divided into two series (A and B) of 7 cards each. The Standard Card was common for the two series. The cards were made of heavy white cardboard, 180 mm. wide and 100 mm. high, on which were pasted vertical bars 4 mm. wide and 50 mm. high.

The Standard Card contained two black parallel bars with a separation of 9 mm. The comparison-cards contained four parallel vertical bars, the separation between the two outer pairs of bars remained constant at 9 mm., while the separation between the two middle bars varied. The various separations of the middle bars were the same for Series A and B: 7½, 8, 8½, 9, 9½, 10, and 10½ mm.

* Accepted for publication April 6, 1932. This study was suggested and directed by Professor S. W. Fernberger.

[1] H. Helson, The psychology of gestalt, this JOURNAL, 37, 1926, 37.

[2] G. M. Whipple, *Manual of Mental and Physical Tests,* 1910, 227.

Series A and B differed only in the pattern formed by the color of the bars. Series A contained two red and two blue bars, the red to the left and the blue to the right. Series B contained three red bars and one blue bar, the blue being to the extreme right. The judgments required were of the relative separation of the two central bars. In Series A the judgments were between the inner bars of the two color pairs while in Series B the judgments were between the two center bars of the red group. In Series A the distances to be judged lay between two spontaneous configurations, while in Series B they lay within a spontaneous configuration formed by the color of the bars.

Procedure. *O* sat in a comfortable chair 7 ft. in front of the tachistoscope. The chair was of a height to bring his eyes on a level with the exposure-field. In order to escape the undesired effects of fatigue, *O* was permitted to rest whenever he desired. Whether he had availed himself of this privilege or not, a 5-min. rest period was given him at about the middle of every experimental hour.

The cards of the two series (A and B) were intermixed, and presented haphazardly. In order to minimize the influence of the serial arrangement, 10 different haphazard orders were employed during the course of the study. But one time order was used in presenting the cards. The standard was always presented first, and then the variable. The comparisons were made as rapidly as possible. After some practice *E* was able to give the *O*s 14 comparisons on an average, in a 5-min. work period —approximately 1 every 20 sec.

The *O*s were permitted to write down their casual introspections at the end of every experimental hour.

Observers. The *O*s,[3] 5 in number, where Dr. Stanley F. Cooper (*C*), Dr. John F. Humes (*H*), Dr. Francis W. Irwin (*I*), members of the Departmental Staff, and Mr. Wendell R. Carlson (*K*), and Mr. Charles C. Limburg (*L*), graduate students. All possessed normal or normally adjusted vision. Three of them (*C, H,* and *I*) were acquainted with tachistoscopic procedure while for 2 (*K* and *L*) it was new. All were unacquainted with the details of the experiment except those given in the instructions and apparent in the procedure. Each served for a period of about an hour, on 2 days a week, for 15 weeks, extending from February to June, 1931.

Every *O* gave a total of 3,500 judgments: 250 on each of the 14 comparison-cards —25 for each of the 10 haphazard orders.

Instructions. The following instructions were read at the beginning of each experimental session.

I am going to show you a number of cards. First I shall show you a standard card of two bars, then a comparison-card of four bars, then another standard card and another comparison-card and so on. I want you in each case to pay careful attention to the standard card and then give me your judgment for the following comparison-card. The standard card may vary as well as the comparison-cards. I want you to judge whether the central bars of the comparison-card are the same distance apart as the bars of the standard or whether the distance apart is greater or less. After each comparison-card you will give me your judgment, merely saying the words 'less,' 'equal,' or 'greater.' Be sure in each case to pay careful attention to the standard card so that you may give a true comparison judgment. Before presenting a standard card I will say the words 'standard' and 'now' with a short pause between. Before presenting a comparison-card I will say the words 'ready' and 'now' with a short

[3] I wish to express my thanks to the *O*s for their assistance and coöperation.

pause between. I want your judgments on the comparison-cards to be immediate. Just say 'less,' 'equal,' or 'greater.'

RESULTS

For every *O*, 25 judgments were obtained on the 14 comparison stimuli for each of the 10 haphazard orders of presentation. Consecutive fractions were paired for purposes of calculation. From these 5 combined fractions, measures were computed separately for Series A and B in accordance with the $\Phi(\gamma)$ hypothesis making use of Urban's tables.[4]

These computed measures are shown in Tables I to X, all of which are similar. There are 2 tables for every *O*. The odd numbered tables show results for Series A, the even for Series B. In the first column of each of the 10 tables is shown the combined consecutive fractions for which the meas-

TABLE I

C's RESULTS IN SERIES A

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	0.83	1.19	8.46	9.41	0.95	9.02
3 & 4	0.91	1.02	8.45	9.55	1.11	9.03
5 & 6	1.07	1.17	8.17	9.30	1.13	8.76
7 & 8	0.82	1.09	8.16	9.33	1.17	8.83
9 & 10	1.20	1.18	8.28	9.27	0.99	8.77
Av.	0.97	1.13	8.30	9.37	1.07	8.88

TABLE II

C's RESULTS IN SERIES B

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	0.80	0.89	8.59	9.45	0.86	9.04
3 & 4	0.81	1.15	8.46	9.43	0.97	9.03
5 & 6	0.88	1.13	8.18	9.26	1.09	8.79
7 & 8	1.34	1.18	8.07	9.30	1.23	8.65
9 & 10	1.02	1.14	8.23	9.36	1.13	8.83
Av.	0.97	1.10	8.31	9.36	1.06	8.87

TABLE III

H's RESULTS IN SERIES A

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	0.57	0.55	8.66	9.23	0.56	8.94
3 & 4	0.71	0.69	8.76	9.02	0.26	8.89
5 & 6	0.65	0.62	8.93	9.15	0.22	9.04
7 & 8	0.52	0.52	8.89	9.25	0.36	9.07
9 & 10	0.77	0.78	8.92	9.07	0.15	8.99
Av.	0.64	0.63	8.83	9.14	0.31	8.99

[4] See S. W. Fernberger, A simplification of the practice of the method of constant stimuli, this JOURNAL 25, 1914, 121-130. Also G. J. Rich, A checking table for the method of constant stimuli, *ibid.*, 29, 1918, 120-121.

TABLE IV

H's Results in Series B

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	0.50	0.54	8.54	9.17	0.63	8.86
3 & 4	0.67	0.63	8.64	8.94	0.31	8.78
5 & 6	0.60	0.59	8.76	8.98	0.22	8.87
7 & 8	0.53	0.50	8.63	9.01	0.39	8.81
9 & 10	0.68	0.75	8.91	9.13	0.23	9.03
Av.	0.60	0.60	8.70	9.05	0.36	8.87

TABLE V

I's Results in Series A

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	1.25	1.28	8.56	9.36	0.80	8.97
3 & 4	1.46	0.88	8.56	9.15	0.59	8.78
5 & 6	1.87	1.19	8.51	9.36	0.85	8.84
7 & 8	1.53	0.91	8.59	9.42	0.84	8.90
9 & 10	1.87	1.47	8.69	9.46	0.77	9.03
Av.	1.60	1.15	8.58	9.35	0.77	8.90

TABLE VI

I's Results in Series B

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	1.11	1.30	8.51	9.18	0.67	8.87
3 & 4	1.44	1.17	8.47	9.10	0.62	8.75
5 & 6	2.14	1.31	8.43	9.33	0.90	8.77
7 & 8	1.06	1.13	8.61	9.43	0.82	9.03
9 & 10	1.39	1.56	8.72	9.55	0.83	9.16
Av.	1.43	1.29	8.55	9.32	0.77	8.92

TABLE VII

K's Results in Series A

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	1.19	1.20	8.53	9.56	1.03	9.05
3 & 4	1.60	1.05	8.64	9.80	1.16	9.10
5 & 6	1.29	1.09	8.70	10.02	1.31	9.30
7 & 8	1.08	1.31	8.81	10.16	1.35	9.55
9 & 10	1.35	1.43	8.70	9.87	1.17	9.30
Av.	1.30	1.22	8.68	9.88	1.20	9.26

TABLE VIII

K's Results in Series B

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	0.94	1.82	8.45	9.39	0.94	9.07
3 & 4	1.35	1.60	8.45	9.63	1.18	9.09
5 & 6	1.69	1.54	8.56	9.86	1.29	9.18
7 & 8	1.22	1.91	8.55	9.92	1.37	9.39
9 & 10	1.30	1.13	8.68	9.99	1.31	9.29
Av.	1.30	1.60	8.54	9.76	1.22	9.20

TABLE IX

L's Results in Series A

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	1.17	0.94	8.27	9.83	1.56	8.97
3 & 4	1.25	1.15	8.33	9.88	1.54	9.07
5 & 6	0.84	1.11	8.26	10.17	1.90	9.34
7 & 8	0.90	1.08	8.04	10.14	2.10	9.18
9 & 10	1.31	0.86	8.07	9.90	1.83	8.80
Av.	1.09	1.03	8.19	9.98	1.79	9.07

TABLE X

L's Results in Series B

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	1.01	1.06	8.25	9.67	1.43	8.98
3 & 4	1.27	1.15	8.23	9.61	1.38	8.88
5 & 6	1.34	1.20	8.10	9.77	1.67	8.89
7 & 8	1.05	1.16	7.92	9.88	1.96	8.95
9 & 10	1.33	1.07	8.17	9.84	1.68	8.91
Av.	1.20	1.13	8.13	9.75	1.62	8.92

ures were calculated. In the second and third columns are shown the Coefficients of Precision for the 'Less' and 'Greater' judgments, h_1 and h_2 respectively. In the fourth and fifth columns are shown the thresholds for the 'Less' and 'Greater' judgments, S_1 and S_2 respectively. The sixth column shows the values of the Interval of Uncertainty, and the seventh column shows the Point of Subjective Equality.[5] The average for each of the measures is shown in the bottom row of the table.

In Figs. 1 to 5 are shown straight line graphs of the judgments of every *O* for the entire experiment obtained by combining the judgments for all 10 fractions. The curves for the 'Less,' 'Equal,' and 'Greater' judgments are readily distinguishable. It will be noted that the curves are relatively of the normal $\Phi(\gamma)$ type. The values for Series A are connected by continuous lines, while those for Series B are connected by broken lines. It will be noted that there is little difference between the results of the two series. The curves for *C*, *H*, and *I* practically coincide in the two series, and those for *K* and *L* are very like.

The Intervals of Uncertainty are the most crucial values for the purposes of our experiment as they are the chief measures of the accuracy of judgment. Let us consider them in more detail. Table XI gives these values with their Probable Errors for all the *O*s for each series, together with their dif-

[5] For an explanation of these measures see S. W. Fernberger, On the relations of the methods of just perceptible differences and constant stimuli, *Psychol. Monog.*, 14, 1913, (no. 61), 29-49.

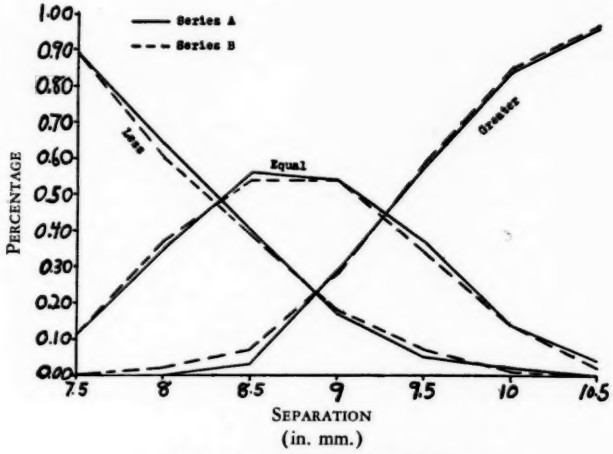

FIG. 1. CURVES SHOWING *C*'S RESULTS

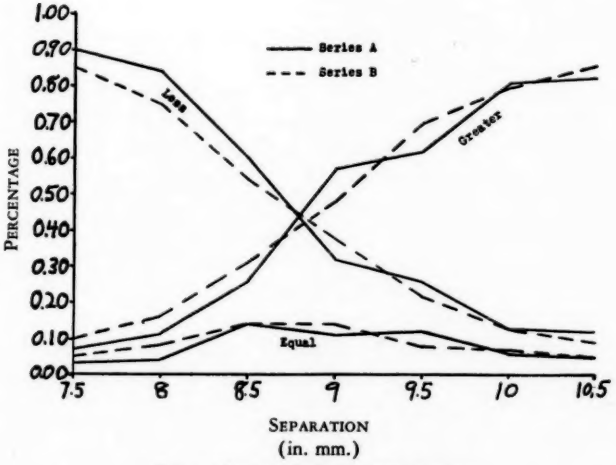

FIG. 2. CURVES SHOWING *H*'S RESULTS

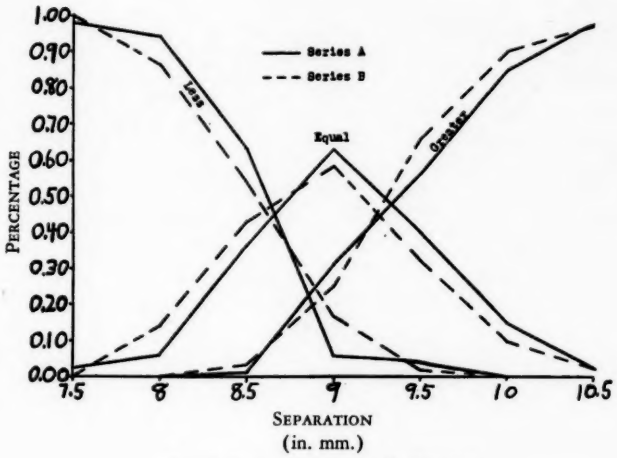

FIG. 3. CURVES SHOWING *I*'S RESULTS

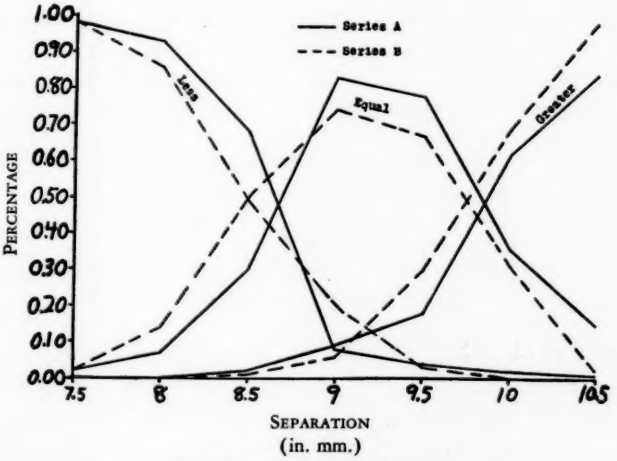

FIG. 4. CURVES SHOWING *K*'S RESULTS

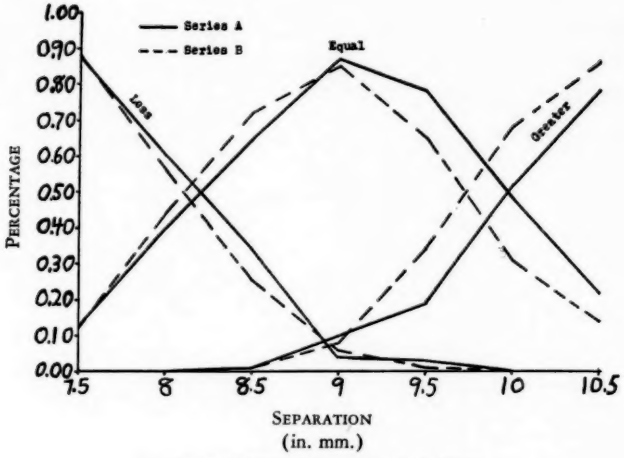

FIG. 5. CURVES SHOWING *L*'S RESULTS

ference and an index of the significance of that difference.[6] One of the *O*'s, (*I*), shows no difference between the intervals of uncertainty, and two of them (*C* and *L*) show a difference in one direction, and two (*H* and *K*) show a difference in the opposite direction. The differences are all small and the indexes of reliability are likewise small ranging from 0.1031 for *C* to 0.7485 for *L*. These values, even *L*'s the largest, are too low to indicate any real significance. Such difference as we found must be regarded, therefore, as being due to chance.

Not only do the size and direction of the average interval of uncertainty vary among the different *O*s, but they also vary in the results of the in-

TABLE XI

COMPARISON OF THE AVERAGE INTERVALS OF UNCERTAINTY FOR EVERY *O*

	Interval of Uncertainty					
	Series A		Series B			
O	Av.	P.E.	Av.	P.E.	Diff. (A–B)	Significance of Differences
K	1.20	.0384	1.22	.0513	−0.02	.1667
C	1.07	.0283	1.06	.0432	+0.01	.1031
H	0.31	.0479	0.36	.0506	−0.05	.3699
I	0.77	—	0.77	—	0.00	—
L	1.79	.0715	1.62	.0701	+0.17	.7485

[6] For the determination of this index see S. W. Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 3, 1920, 141-142.

dividual *O*s. As shown in Tables I-X, where the data are fractionated for every *O* into 5 equal parts, the intervals of certainty for a given *O* are sometimes larger for Series A and sometimes for Series B. For *L* alone do the differences between comparable fractions all lie in the same direction, *i.e.* toward Series A. *H* is the next most consistent. In 4 of the 5 fractionations, Series B is the larger, and in the fifth fraction the two series are equal. The data of the other 3 *O*s, however, show considerable fluctuation. The intervals of uncertainty are larger in Series A for 3, 2, and 2 of the 5 fractionations for *C, I,* and *K* respectively.

From a consideration of all these facts it is evident that any conclusion as to a definite and statistically significant difference between the Intervals of Uncertainty for the two configurations would be unwarranted. This means that the two configurational arrangements in Series A and B exerted no differential effect on the ability to judge differences in the two cases under our experimental conditions.

The Points of Subjective Equality for Series A and B as shown in Tables I to X show no uniformity in their relation to actual equality. There appears to be no consistent time error, nor is the approximation to actual equality consistently closer for one series than for the other.

From a consideration of the size of the Coefficients of Precision for Series A and B, as indicated in Tables I to X, we again find no consistent difference. Also, there is no uniform tendency for the Coefficients of Precision to become larger or for the Intervals of Uncertainty to become smaller in the later fractions as compared with the earlier fractions. This would be expected if improvement due to progressive practice took place in the accuracy of judgments.

Our findings in this experiment have indicated no differential effect for the two configurations used in Series A and B. Moreover, there does not seem to have been any added effort required to judge one series as compared with the other. At least the occasional recorded introspections do not reveal such a condition. The one configurational arrangement seems to have given as accurate results and presented no more difficulty of any sort than the other.

These findings are not in accordance with the results expected at the outset of the experiment from other studies of the influence of configurations upon thresholds. We had expected that the configurational nature of Series B would facilitate judgments and that of Series A would tend to impair the accuracy of judgments. Series B, as we have pointed out, involved judgments within a dominant spontaneous configuration, while Series A involved judgments between two dominant spontaneous configurations. The organi-

zation of the two central bars required for judgments under the conditions of this experiment should be favored by the former configuration and directly opposed by the latter configuration.

Conclusions

From this experiment we are led to conclude one of three things: (1) either configurational membership exerts no influence upon the thresholds for the judgment of the separation of two objects visually presented; (2) or the conditions of our experiment were inadequate to detect such influence actually present; (3) or our interpretation of the nature of our stimuli was incorrect due to the inadequacy of color alone to materially effect configurational organization.

Summary

(1) Thresholds were obtained by the method of constant stimuli for the judgment of the separation of bars in two different visual Gestalten, one of which demanded judgments within a dominant spontaneous configuration and the other of which demanded judgments between two dominant spontaneous configurations. The configurations were determined by the color of the component bars, red and blue being used in two different arrangements.

(2) A treatment of the judgments of the two configurational arrangements gave similar results. No differential effect was apparent for the two configurational arrangements for the Intervals of Uncertainty, the Points of Subjective Equality, or the Coefficients of Precision. The results for the two configurational arrangements showed no consistent or significant differences.

A METHOD OF MEASURING THE EFFECT OF PRIMACY OF REPORT IN THE RANGE OF ATTENTION EXPERIMENT

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The traditional 'range of attention' experiment consists of the flashing before the eyes of the *S*, by means of a tachistoscope, a card bearing a number of letters of the alphabet, colors, dots, forms, or lines. The flash consumes a fraction of a second, usually 100 σ. Immediately afterward *S* writes out or expresses orally the contents, in proper order, of what he believes he perceived during the flash. This act of writing or speaking or reproducing psychologists have termed the *S*'s 'report.' In order to make such a 'report,' *S* represents to himself, to his mind, by visual imagery or in some other manner, the letters or other contents prior to or during the act of writing or speaking. The latter, this act of mental representation to make possible the 'report,' psychologists have called the act of 'recall.' During the flash *S* perceived many of the exposed contents, some much more clearly than others. The different degrees of clearness in the perceptual act, have been described as an exhibition of 'attention.' For brevity's sake the 'seeing' of the contents during the brief exposure is called 'perceptual attention.' The purpose of the experiment is to measure, by measuring the number of the contents of 'recall' and 'report,' the 'range' or number of contents which were perceived with such a high degree of clearness that their 'report' was certain. This experiment involves at least three variables: (1) the act of *perceptual 'attention,'* causing certain contents to stand out dominantly over others during the brief exposure; (2) the act of *'recalling,'* through imagery and otherwise, what one 'remembers' of the contents which were flashed a moment previously; and (3) the act of *'reporting,'* in writing or spoken words, what one is 'recalling' or has just 'recalled.' The variable processes of 'recall' and 'report' are known to function with a high frequency from left to right in a majority of *S*s. Consequently if the results of 'reports' are plotted in a graphic form, say for the content positions from left to right (1 to 9, for cards carrying 9 contents arranged in a horizontal sequence) a high 'recall' or 'report' efficiency characterizes the leftmost position (position no. 1). From there on toward the right up to the 6th and 7th positions, a rapid decline of effectiveness is to be observed. The final or rightmost content will show an increased effectiveness: the curve in that region will rise somewhat instead of remaining parallel with the base line or declining further. Such a graph of the effectiveness of the 'recalls' or 'reports' for the various letter-positions from left to right, is very similar to the typical learning curve.

But several questions obtrude immediately. Is this decline due to the perceptual and 'attention' process during the exposure? Is it due to the priority or primacy of 'recall?' Finally, is it due to primacy of both 'recall' and 'report?' Psychologists have for many years doubted the efficacy of this experiment to reveal the 'range of attention.' It certainly does bring out some of the effects

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of the range of 'memory.' Can the latter be conceived as a co-variable of the 'range of perceptual attention?' The answer has been in many quarters,[1] a vigorous "no." Indeed, psychologists for many decades have considered particularly baffling this problem of distinguishing in 'range of attention' scores between the effects of 'perceptual attention' during exposures and the effects of 'recalling' and 'reporting' what was perceived. In a recent paper the senior author stated a belief that the problem had never been solved successfully.[2] The hope was there expressed that soon a solution would be found which would enable us properly to evaluate the effects of the primacy of 'report' in this type of experiment. Believing that an extended investigation on 225 *Ss* undertaken last year and being continued now has given at least a partial solution to the problem, we wish to present the technique and many of its results for criticism or re-application by other investigators.

Preliminary Experiment

Our first effort to solve the problem developed from the following line of reasoning. If during tachistoscopic exposure perceptual 'attention' is distributed from left to right, having its maximum clearness near the first letter-position, and if the usual type of 'recall' has its own order of effectiveness, as determined by primacy from left-to-right, why cannot the two sequences of clearness or the two orders of effectiveness of 'perception' and 'recall,' be caused to oppose each other? If perception proceeds from left to right, why should not an *S* be forced to 'recall' in an order from right to left, that is, in an order quite the opposite to the order of 'recall' to which most *Ss* are habituated? If such reverse order of 'recall' be possible, and if the effect of 'recall'-order is considerable, why would not the effectiveness of the 'perceptual'-order of the clearness of exposure cancel out or balance the effectiveness of a reverse order of 'recall,' thus to render the letter-position curve a straight line, parallel to the base line? If one or the other factor is the stronger, why will not the effect of the stronger cause this curve to vary its direction of declination, especially in those cases where the *fixation* of perception can be made to conflict with the order of 'recall' stipulated, as in left-right perceptions with right-left 'recalls' and in right-left perceptions accompanied by left-right 'recalls?' And, if it be possible to cause *Ss* to modify the habitual order of 'recall,' to produce right-left reproductions, why cannot a 'recall' starting at the center character and proceeding thence in alternate fashion towards the left and the right be also possible of attainment?

Plans were formulated to test out these proposals. The preliminary experiment was tried on 25 sophomore college students. Home-made, gravity tachistoscopes were employed.[3] The laboratory's traditional exposure cards served as exposure materials.[4] These were printed in 18-pt. Caslon bold capitals, 4 to 10 letters in

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[1] See especially the many papers by K. M. Dallenbach and his students, many of which have been reviewed in *Psychol. Bull.*, 23, 1926, 1-18; 25, 1928, 493-512; 27, 1930, 497-513.

[2] H. R. Crosland, Letter-position effects in the range of attention experiment as affected by the number of letters in each exposure, *J. Exper. Psychol.*, 14, 1931, 477-507. See especially 501-503, and 507.

[3] These were described in *J. Exper. Psychol.*, 14, 1931, 479, 481.

[4] *Loc. cit.*

nonsense arrangement in the center of each card. Specially mimeographed recording blanks were supplied the *Ss*. After each exposure, *E* presented a blank indicated by an arrow where the 'recall' should start and the order in which it must progress. Oral instructions stipulated before each exposure of 100 σ that fixation must be held upon the center of the field. The order of the 'recall' was withheld until after the exposure had been effected. The two new orders of 'recall,' added to the usual left to right (L-R) order, were right to left (R-L), and center-alternate (C-A).

This preliminary experiment proved to be a flat failure. In a short time we discovered that instructing an *S* to 'recall' in a given order was no guarantee that he could carry out the instructions. A distinction between order of 'recall' and order of 'report' became at once necessary. The students testified that they could not resist, check, or transform their habituated left-to-right orders of 'recall;' that greatest difficulty attended their efforts to make order of 'report' differ from order of 'recall;' and that by the time a R-L or C-A 'report' was under way they had forgotten the 'recalled' letters. Indeed when these unusual 'reports' had been accomplished, after a fashion, the 'range' scores so returned were for the various letter-positions not appreciably larger than zero. In other words, the carrying through of the instructions to 'report' in the R-L and C-A orders caused so much forgetting of the letters as 'recalled' that the earned 'range' scores were worthless for the purposes of the experiment.

The failure of the preliminary experiment was disheartening. Though difficult to understand, it nevertheless taught two lessons: 'report' might be controlled, but 'recall' would continue to be refractory, and if the technique was to succeed, it could do so only after the *Ss* had submitted to large amounts of practice in the odd, R-L and C-A orders of 'report.'

Main Experiment

Technique. An experiment to afford preliminary training to 120 *Ss* and after that to test the influences of the three orders of 'report,' was immediately begun. Its formulation included plans to control additional variables which we had not hitherto commanded.

(1) Fixation. The first and foremost of these was fixation of the *S*'s eyes during an exposure. Ten expedients were designed to control, or to measure the effects of, fixation.

(1) The form of print. Fifty-four cards, each 114 mm. by 94 mm. in the vertical and horizontal dimensions respectively, were printed in 14 pt. lower-case Caslon bold type. The 54 were divided into two series, a practice and a main series, of 27 cards each. Each card carried 9 letters in nonsense arrangement. The letters were printed exactly 40 mm. from the bottom of the card. The entire 54 were divided into 3 sets, depending upon the location of the 9 letters across the card from left to right. Eighteen of the cards carried letters placed in the center, that is, with the fifth or middlemost letter placed in the center of the card (between left and right). These are called 'Center-Prints.' Another 18 carried 'Left-Prints,' whose rightmost or 9th characters lay just at the center of the card. The remaining 18 carried 'Right-Prints;' their leftmost or 1st characters were located at center. One purpose of the variable method of print was to procure different fixations, when *S* was required always to fixate the center of the field and the center of the card. Another purpose was to encourage central fixation, for without it *S* would frequently 'see' nothing but white cardboard during an exposure.

(2) Random or chance presentations of 27 cards carrying the various kinds of print.
(3) Two hundred preliminary trials, to habituate central fixation and to habituate the two unusual orders of 'report' (R-L and C-A)—100 trials each.
(4) The 3 orders of 'report,' of L-R, R-L, and C-A—in the main exposures proper.
(5) A random order of these 3 kinds of 'report.'
(6) A chance or random sequence of combinations of *print* of exposure and order of *'report.'*
(7) Introspections as to where, during exposure, fixation was located.
(8) Preëxposure fixation of a hair-line cross (x) in the center of the field.
(9) Typed instructions, reiterated before every exposure, urging the necessity of central fixation; in fact, no exposure was made until *S* indicated that he had fixation adjusted as instructed.
(10) An exposure period of only 100σ.

(2) Handedness. The second variable controlled was that of handedness.[5] Sixty of the 120 *S*s were right-handed; the rest were left-handed.

(3) Eyedness. Eyedness was under control: 60 *S*s were right-eyed and 60 left-eyed.

(4) Handedness and eyedness. Handedness and eyedness were also controlled: 30 *S*s were right-handed and right-eyed; 30 were right-handed and left-eyed; 30 were left-handed and left-eyed; and the remaining 30 were left-handed and right-eyed.

(5) Sex. Each of the above groups of 30 *S*s contained 15 men and 15 women; thus sex was controlled.

So, omitting reference to these collateral devices introduced merely for purposes of control, one observes that the main experiment had two purposes: (1) the affording to each *S* of 100 trials for each of the two unusual orders of 'report' in *right-left* and *center-alternate* directions; and (2) the measurement of the comparative effects of 3 types of 'report,' L-R, R-L, and C-A, under each condition of print (fixation).

In both the trial and main-exposure series a specially mimeographed recording blank was employed. It carried (after a 'report' had been recorded) records concerning measurements of handedness, eyedness, sex, etc. Nine blanks were used for the entering by *E* of the letters as objectively correct. Nine other blanks, just under these, were filled in by *S*, just after an exposure. An arrow indicated where he was to begin his report and the direction it was to take. *E* also gave oral instructions as to order of 'report' just after an exposure ended. *S* did not see this arrow, or know what order of 'report' to expect, until after the exposure had ended. *E* stood at the elbow of his writing arm to assure that he would obey instructions. In case introspections or the discovery of white cardboard by him indicated a failure to fixate the center, the record was discarded and the exposure made over again at some other time, in another sequence.

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[5] Handedness was measured by a series of 5 questions taken from Downey's questionnaire on handedness. Three or more answers in the same direction of conformity constituted the standard of classification; and while absolute conformity between the 5 answers was not required, still in 9 cases out of 10 a ratio of 5 to 0 was obtained. Eyedness was measured in four ways: the two-finger, homonymous, heteronymous double-images test; a modification of this; Miles's manuscope; and a variation of the latter.

Introspective data were recorded in the form of answers to the following questions: Where was your fixation directed just prior to the moment when the exposure occurred? At the moment the exposure began where was your fixation directed? Did fixation move or alter its position after the exposure occurred, that is, during the duration of the exposure itself? If so, how many times did it so alter or change? If it altered, from what point to what point or points did it move, so far as you can now recall? Did any of the letters come to your mind, during the writing, in a different order than the one you were required to follow in writing? If the 'recall' order differed from that of the 'writing,' please state in detail the order of the recall.

Did any letters occur to you, during the writing itself, which you could *not* recognize as having been seen during the exposure? If so, what in detail were such letters?

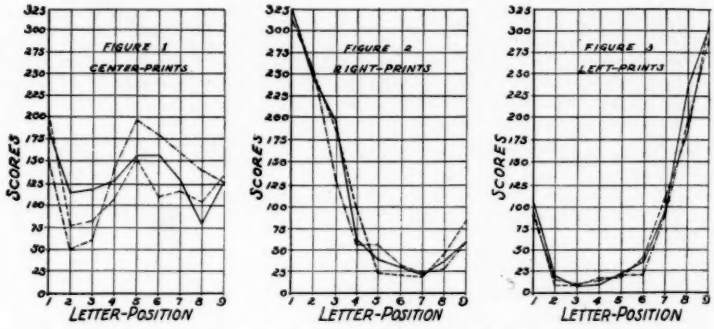
The exposures were made through means of a Dodge mirror-tachistoscope.[6] The time of the exposure was controlled to 100 σ by a modification of the gravity chronoscope.[7]

Not only did this experiment succeed where the preliminary investigation had failed, but its success is exceeded by that of the study now in progress. The 25 preliminary *Ss* either could not 'report' in the two unusual forms, or the scores of such 'reports' approached the magnitude of zero. The 120 *Ss* of the main experiment could not only make, after 200 preliminary trials, sizable scores under R-L and C-A 'reports,' but the R-L 'reports' were as competent as the L-R 'reports,' a fact amply demonstrated by the data to follow below. The 100 or more *Ss* in the current investigation do not require any preliminary training. They make R-L and C-A 'reports' just as easily as they make L-R 'reports.' The success of the main experiment as compared with the preliminary, is due undoubtedly to the thorough control of fixation and to the chance sequences in which 'reports' and 'prints' accompany one another and succeed themselves. *S* cannot anticipate the kind of print (fixation) nor the order of 'report.' Hence his largest chance of obtaining a high score will depend upon fixation of the center of the exposure field. Moreover he has been aided in this type of fixation by the 200 preliminary trials. The success of the current project over the main one of last year lies, we believe, in the fact that the present *Ss* are all defective readers. The nature of this defectiveness does not, however, concern us here.

Results. The above assertions of success will be more convincing when the obtained results are studied. Below are presented 6 graphs (Figs. 1-6) and 7 tables. The former relate to differences between orders of 'report' under each type of print (fixation) and to differences between prints (fixations) under each order of 'report.' The tables relate to the sizes and significances of differences, as between prints, in one case, and between 'reports,' in the other case, where in each instance the 3 samples of the other variable have been pooled; and to the general effectiveness of the L-R, R-L, and C-A types of 'report' where all letter-positions have themselves been pooled.

Scrutiny of Figs. 2 and 3 should serve to convince the most skeptical reader that absolutely no differences appeared as between the three orders of report: right-left, left-right, and center-alternate. Not only do the curves of the effectiveness of report begin at approximately the same high points in each of these

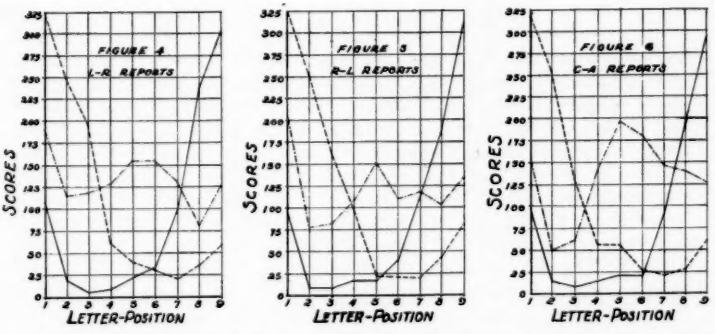
[6] *J. Appl. Psychol.*, 12, 1928, 82-85.
[7] Cf. *J. Exper. Psychol.*, 9, 1926, 162-168.



FIGS. 1-3. SHOWING, FOR EACH CONDITION OF PRINT, THE SCORES FOR EVERY LETTER-POSITION

Center-Print = center-fixation
Right-Print = left-fixation
Left-Print = right-fixation

———— = L-R report
– – – – = R-L report
–.–.–.– = C-A report



FIGS. 4-6. SHOWING, FOR EACH ORDER OF REPORT, THE SCORES FOR EVERY LETTER-POSITION

L-R = left to right report
R-L = right to left report
C-A = center-alternate report

———— = Left-Print
– – – – = Right-Print
–.–.–.– = Center-Print

figures, but the declination is practically of the same form and equal in amount. This is so true that constructing the drawings was very difficult; at many points the 3 curves in each of these figures actually superimpose themselves. The meaning of the approximate identicalness of the 3 'report' curves in each of Figs. 2 and 3 is to the effect that R-L and C-A reports are quite as competent as L-R reports. Moreover, all 3 give exactly the same results, curves of the same

TABLE I

DIFFERENCES BETWEEN THE MEAN SCORES FOR 'LEFT-RIGHT' AND 'RIGHT-LEFT' REPORTS FOR EACH LETTER-POSITION; THE STANDARD ERRORS OF THESE DIFFERENCES, WITH THE SIGNIFICANCE OF EACH DIFFERENCE

(N = 120)

(Maximum Score = 9)

Position	M_1	M_2	D.	σ_1	σ_2	σ Diff.	D/σ Diff.	Percent Chances
1	5.23	5.19	0.04	2.33	2.81	.2	0.2	58
2	2.83	3.17	0.34	1.46	1.55	.142	2.4	99
3	2.11	2.72	0.61	1.46	1.38	.149	4.1	100
4	1.84	1.64	0.20	1.24	1.24	.119	1.7	96
5	1.59	1.83	0.24	1.23	1.41	.133	1.8	96
6	1.53	1.84	0.31	1.33	1.51	.144	2.2	99
7	2.11	2.17	0.06	1.70	1.63	.141	0.4	65
8	2.75	2.94	0.19	1.71	1.44	.155	1.2	88
9	4.38	4.06	0.32	1.83	1.81	.170	1.9	97
Mean	2.71	2.84	0.247				1.77	89

form, same magnitudes, and with the same degrees of declination. Finally, the factor of almost infinitely great importance is the form of fixation as this is determined by the style of print. When one compares Fig. 2 with Fig. 3, one finds curves almost the perfect opposites of one another in the direction and form of declination. The curves of Fig. 3 have their highest scores at position 9; those of Fig. 2 at position 1. Position 1 in Fig. 3 exhibits slightly higher scores than position 9 in Fig. 2. In Fig. 2, position 1 gives slightly higher scores than position 9 in Fig. 3. Positions 5, 4, 3, and 2 in Fig. 3 show slightly lower scores than positions 5, 6, 7, and 8 in Fig. 2. The conclusions from Figs. 2 and 3 deserve emphatic reiteration: (1) R-L and C-A reports can be and are made competently. (2) R-L, L-R, and C-A reports are exactly alike, in nearly every respect, in each figure; and (3) the form of print, the fixation of the eyes, or of 'attention,' is by all odds the chief determiner of the score and of which

TABLE II

DIFFERENCES BETWEEN THE MEAN SCORES FOR 'LEFT-RIGHT' AND 'CENTER-ALTERNATE' REPORTS FOR EACH LETTER-POSITION; THE STANDARD ERRORS OF THESE DIFFERENCES, WITH THE SIGNIFICANCE OF EACH DIFFERENCE

(N = 120)

(Maximum Score = 9)

Position	M_1	M_2	D.	σ_1	σ_2	σ Diff.	D/σ Diff.	Percent Chances
1	4.81	5.19	0.38	2.00	2.81	.1257	3.02	100
2	2.66	3.17	0.51	1.27	1.55	.1536	3.32	100
3	1.70	2.72	1.02	1.35	1.38	.1357	7.52	100
4	1.80	1.64	0.16	1.21	1.24	.1277	1.25	89
5	2.28	1.83	0.45	1.40	1.41	.1175	3.83	100
6	1.88	1.84	0.04	1.38	1.51	.127	0.31	62
7	2.15	2.17	0.02	1.66	1.63	.1296	0.15	56
8	3.11	2.94	0.17	1.60	1.44	.1356	1.25	89
9	3.99	4.06	0.07	1.69	1.81	.1281	0.55	71
Mean	2.70	2.84	0.313				2.36	85

TABLE III

DIFFERENCES BETWEEN THE MEAN SCORES FOR 'RIGHT-LEFT' AND 'CENTER-ALTERNATE' REPORTS FOR EACH LETTER-POSITION; THE STANDARD ERRORS OF THESE DIFFERENCES, WITH THE SIGNIFICANCE OF EACH DIFFERENCE
(N = 120)
(Maximum Score = 9)

Position	M_1	M_2	D.	σ_1	σ_2	σ Diff.	D/σ Diff.	Percent Chances
1	4.81	5.23	0.42	2.00	2.33	.14	3.0	100
2	2.66	2.83	0.17	1.27	1.46	.1304	1.3	90
3	1.70	2.11	0.41	1.35	1.46	.121	3.4	100
4	1.80	1.84	0.04	1.21	1.24	.132	0.3	62
5	2.28	1.59	0.69	1.40	1.23	.122	5.7	100
6	1.88	1.53	0.35	1.38	1.33	.127	2.8	100
7	2.15	2.11	0.04	1.66	1.70	.141	0.3	62
8	3.11	2.75	0.36	1.60	1.71	.168	2.1	98
9	3.99	4.38	0.39	1.69	1.83	.149	2.6	99
Mean	2.70	2.71	0.32				2.36	90

position-scores will be high and which low. Form of print (fixation) renders every order of report into a form consistent with itself. Priority of 'report,' or of 'recall,' or of both 'recall' and 'report' is wholly absent or its effects are negligibly small. The ruling factor is that of fixation.

With respect, however, to Fig. 1, there are slight differences to be noted as between the orders of report. The L-R, R-L, and C-A curves, under the condition of central fixation, do not superpose themselves, although they are greatly similar in form. This similarity is particularly evident in letter-positions 1, 2, and 3. Some similarity also exists at position 5 (the point of fixation), and again at positions 6 and 7. All three curves differ from those which the senior author has published previously,[8] curves which were believed to have been obtained under conditions of central fixation. (Two explanations probably account for the dis-

TABLE IV

DIFFERENCES BETWEEN THE MEAN SCORES FOR 'LEFT' AND 'RIGHT' PRINTS FOR EACH LETTER-POSITION; THE STANDARD ERRORS OF THESE DIFFERENCES, WITH THE SIGNIFICANCE OF EACH DIFFERENCE
(N = 120)
(Maximum Score = 9)

Position	M_1	M_2	D.	σ_1	σ_2	σ Diff.	D/σ Diff.	Percent Chances
1	8.07	2.51	5.56	1.72	2.52	.3553	21.78	100
2	6.30	0.35	5.95	2.29	0.856	.2135	27.87	100
3	4.00	0.19	3.81	2.19	0.959	.2042	18.66	100
4	1.47	0.28	1.19	1.63	0.645	.1487	8.00	100
5	0.99	0.49	0.50	1.36	0.894	.1319	3.79	100
6	0.68	0.78	0.10	1.03	1.29	.1393	0.718	76
7	0.53	2.52	1.99	1.11	2.32	.2138	9.31	100
8	0.88	5.13	4.25	1.40	2.31	.2377	17.88	100
9	1.64	7.57	5.93	2.04	1.67	.2454	24.16	100
Mean	2.73	2.20	3.24				14.69	97

[8] *J. Appl. Psychol.*, 12, 1928, 82-124; *Psychol. Bull.*, 26, 1929, 375-377; *J. Exper. Psychol.*, 14, 1931, 477-507.

TABLE V

DIFFERENCES BETWEEN THE MEAN SCORES FOR 'LEFT' AND 'CENTER PRINTS' FOR EACH LETTER-POSITION; THE STANDARD ERRORS OF THESE DIFFERENCES, WITH THE SIGNIFICANCE OF EACH DIFFERENCE
(N = 120)
(Maximum Score = 9)

Position	M_1	M_2	D.	σ_1	σ_2	σ Diff.	D/σ Diff.	Percent Chances
1	2.51	1.59	0.94	2.52	3.21	.2107	4.37	100
2	0.35	2.02	1.67	0.856	1.87	.1559	10.77	100
3	0.19	2.19	2.00	0.959	1.90	.1780	11.24	100
4	0.28	3.13	2.85	0.645	1.87	.1794	15.89	100
5	0.49	4.20	3.71	0.894	2.37	.2128	17.43	100
6	0.78	3.73	2.95	1.29	2.38	.2209	13.35	100
7	2.52	3.33	0.81	2.32	2.43	.2474	3.27	100
8	5.13	2.69	2.44	2.31	2.46	.3095	7.88	100
9	7.57	3.23	4.34	1.67	2.69	.2864	15.15	100
Mean	2.20	2.90	2.41				11.04	100

TABLE VI

DIFFERENCES BETWEEN THE MEAN SCORES FOR 'RIGHT' AND 'CENTER PRINTS' FOR EACH LETTER-POSITION; THE STANDARD ERRORS OF THESE DIFFERENCES, WITH THE SIGNIFICANCE OF EACH DIFFERENCE
N = 120)
(Maximum Score = 9)

Position	M_1	M_2	D.	σ_1	σ_2	σ Diff.	D/σ Diff.	Percent Chances
1	8.07	1.59	6.48	1.72	3.21	.2678	24.20	100
2	6.30	2.02	4.28	2.29	1.87	.2694	15.89	100
3	4.00	2.19	1.81	2.19	1.90	.2462	7.35	100
4	1.47	3.13	1.66	1.63	1.87	.2069	8.02	100
5	0.99	4.20	3.21	1.36	2.37	.2339	13.72	100
6	0.68	3.73	3.05	1.03	2.38	.2089	14.60	100
7	0.53	3.33	2.80	1.11	2.43	.2234	12.53	100
8	0.88	2.69	1.81	1.40	2.46	.199	9.10	100
9	1.64	3.23	1.59	2.04	2.69	.2114	7.52	100
Mean	2.73	2.90	2.96				12.55	100

TABLE VII

MEAN SCORES, OF ALL NINE LETTER-POSITIONS, FOR ALL Ss (120), FOR EACH TYPE OF PRINT (FIXATION), ARRANGED BY ORDERS OF REPORT
(Maximum Score = 3)

Order	Left-Print	Center-Print	Right-Print	Mean
L–R	.766	1.114	.932	.937
R–L	.733	1.012	.952	.899
C–A	.698	1.106	.879	.894
Mean	.732	1.077	.921	

cordant results: failure in the early experiments to control fixation, and the fact that practically all early *Ss* were both right-eyed and right-handed.) The older curves declined rapidly from left to right, with no further decline at positions 6 and 7 and with rises for positions 8 and 9. The present curves resemble them only for positions 1 and 2; thereafter all similarity ceases. In fact the high peaks at position 5, and the general greater efficiency of reports of positions 6, 7, 8, and 9, combine with the facts of Figs. 2 and 3 to demonstrate the imperativeness of fixation as the chief factor determining the sizes of scores. One may say that the earlier discovered L to R tendency is somewhat represented at two points in the curves of Fig. 1: from position 1 to position 4, and from 5 or 6 to position 9. Since only a small if any difference exists as between the three orders of report, the earlier-described L to R tendency of 'attention' is still evident in the results. It has been interrupted merely by a stronger factor; namely, that of fixation. In this case it has suffered adversely in competition with central-fixation. It appears also that reports of characters to the right of center, in positions 6, 7, 8, and 9, have been more competent than for positions 1, 2, 3, and 4. The total scores for these positions (all reports pooled) are 1556 and 1428 respectively. This again may be considered as an indication of the presence, though in weakened form, of the 'attentional' and perceptual L to R tendency. The L-R order of report is throughout Fig. 1 slightly superior to the R-L order. If we omit reference to position 1, the total scores for the remaining 8 positions are, for L-R, 1013; for R-L, 887. Similarly omitting the first 5 positions, the scores for positions 6 to 9 are, L-R, 498; for R-L, 468. Observing Table VII, one discovers that under central fixation (center-print), the entire set of 9 positions gives the following average scores: L-R, 1.114; R-L, 1.012; and C-A, 1.106. The latter quantities mean that while L-R and C-A reports excelled R-L reports the differences between them are insignificantly small, even under conditions of central fixation. Hence except for the two exhibitions in Fig. 1, of the L to R perceptual 'attention' tendency, the results in these curves conform closely with those contained in Figs. 2 and 3.

Turning our attention now to Figs. 4, 5, and 6, where the curves above described have been regrouped, we discover at once and unmistakably that the prime factor determining the sizes of position scores is the form of print or fixation. In Fig. 4 the 3 L-R report-curves are almost wholly different from one another. There are only two similarities between them: the initial drop between positions 1 and 2, and the final rise, between positions 8 and 9. Again it is clear that L-R reports were not only not stereotyped in the L to R form but were so fluid as to permit the factor of fixation to cause them to resemble greatly the R-L and C-A reports. Similarly, in Fig. 5, one notes that not only were R-L reports possible but with minor exceptions they resemble very closely the L-R curves of Fig. 4. They were also practically as efficient as the L-R reports. Lack of stereotypy is to be discovered in them; they altered their courses with apparent ease to conform to the condition of fixation (print). Finally the curves of Fig. 6 are very similar to the curves of Figs. 4 and 5. Center-alternate reports conformed so closely to the condition of print or fixation that their curves assumed the forms of the curves of both the L-R and R-L reports. Their largest failure to conform lay in positions 4, 5, and 6, where one observes a fairly high peak in the curve for center-

print. In other words, the order of report in this instance exerted a very small influence when, and only when, it was aided by the factor of fixation. In conclusion it must be reiterated that the order of report is a very weak factor in determining the sizes of position scores. If any factor of 'recall' order was also present, its effects certainly were lost in the effects accruing from the form of fixation; and where it conflicted with the R-L and C-A report orders, it most assuredly was too weak to transform the latter types of report into a form consistent with its own order of effectiveness. The results strongly suggest that the factor of priority or primacy of 'recall,' or of 'report,' or of both 'recall' and 'report,' has been grossly exaggerated in importance. The main factors influencing 'report' efficiency and distributing the sizes of scores among the letter-positions appear to have been inherent in the act of fixation and the perception and 'attention' accompanying such fixation.

There remain to be discussed the contents of Tables I to VII. The first three of these, Tables I, II, and III, show the sizes of differences between the L-R, R-L, and C-A reports for each letter-position, and the significance of each such difference. It should be borne in mind that these tables contain composite results, all forms of print (fixation) pooled together in the mean scores. Hence, should differences between prints prove to be large and significant beyond question, such differences will contaminate those differences apparently accruing from the form of report, to render them larger than they really are and to augment their significance. With this caution, we may proceed to observe the contents of these tables. The mean difference, for the 9 letter-positions, between L-R and R-L reports is only 0.247 (of a maximum score of 9); and the mean significance of the 9 differences is represented by only 89 chances in 100, a number of chances quite under a number which would make the significance certain. This means that many chance factors are present to aid in causing the 9 differences between L-R and R-L reports here shown. When it is recalled that these differences are contaminated by print-differences, one must conclude that such differences are even farther removed from being real differences, and that the number of chance factors is larger than here shown. In only one letter-position (position 3) did a difference occur whose magnitude was large enough to suggest a significance of unquestioned certitude. Likewise, in Table II the mean difference for the 9 letter-positions, between L-R and C-A reports, was only 0.313; with a mean significance of only 85 chances in 100. Considering these facts in the light of the above-mentioned contamination, these differences probably are determined in a large degree by chance factors. Table III shows a mean difference between R-L and C-A reports for the 9 letter-positions of only 0.32, with a mean significance indicated by 90 chances in 100, the largest obtaining in these three tables. In four instances, letter-positions 1, 3, 5, and 6, the significance of the differences appears to be certain, but the aforesaid contamination is probably a contributing factor in the sizes of these differences. Chance factors have not been wholly eliminated from the differences as between R-L and C-A reports as shown in Table III.

Turning to Tables IV, V, and VI, we discover a wholly different state of affairs: the differences between forms of print, that is, between the scores resulting from three types of fixation, are with one exception enormously greater than the differences between orders or report. The exception is for letter-position

6, Table IV, in the differences between 'Left' and 'Right Prints.' Every other difference between forms of 'Print' is so large as to be expected in 100 chances in 100. In fact the ratios of the difference to σ of the difference, range (with the above exception) from 3.27 to 27.87. Inasmuch as a ratio of 3.00 determines perfect certitude of significance, the excess amounts in these ratios above 3.00, are not reflected in additional chances: there is no higher percentage than 100 in 100. If we were to compare the mean ratios of difference to σ difference, of Tables I, II, and III as against Tables IV, V, and VI, we should find 2.16, and 12.76 respectively. This large difference between ratios finds no fair exemplification in degree of significance in favor of the differences between 'prints.' However, the sizes of the differences between 'prints' may be compared with the sizes of the differences between orders of report. The mean difference between reports is 0.293, while the mean difference between 'prints' is 2.87. That is, the latter is 9.6 times as large as the former; and the former differences are contaminated by the latter. We must conclude, therefore, that 'print' or 'fixation' is almost an infinitely more important factor than order of 'report'; that few, and small, if any, differences in sizes of scores result from different types of report.

Scrutiny of Table VII should allow us to generalize concerning differences as between 'prints' and as between 'reports' when all 9 letter-positions have been averaged together. Very little, if any, difference is to be found between the types of 'report' under the condition of 'center-prints.' All 'reports' under this condition were more excellent than those under 'right' and 'left-prints.' Under 'center-print,' L-R reports led, followed in order by C-A, and lastly by R-L reports. The poorest 'reports' occurred under 'left-prints.' Under 'right-prints,' the C-A report was the most inefficient. Under 'left-print,' the L-R report excelled the R-L report. Under 'right-print' the reverse was true: R-L reports excelled the L-R reports. Throughout the entire experiment, where all 'prints' are pooled, the L-R reports are somewhat better than the other two forms; and practically a tie exists between R-L and C-A reports. The only evidences in the whole experiment of the existence of a somewhat stereotyped and fixed or perseverative L to R tendency of 'recall,' or 'report,' or both, are this slight superiority of L-R reports over the other two types when all 'prints' and all letter-positions are pooled together, and the slight inferiority of all reports of 'left-prints.' It is not impossible, however, that the excessive left-eyedness of the 60 left-eyed *Ss*, in contrast with the weaker right-eyedness of the 60 right-eyed *Ss*, weighted very heavily these reports of 'center-' and 'right-prints,' so that the L-R reports seem more incompetent (especially under 'right-prints') than the mere order of report can itself explain. It is a fact that the left-eyed *Ss* were much more at a disadvantage under conditions of 'left-print' than were the right-eyed *Ss* under conditions of 'right-print.' Conversely, left-eyed *Ss* possessed a greater advantage in 'right-prints' than that exhibited by right-eyed *Ss* in 'left-prints.' This is, however, another story, and need not engage us here.

General Summary

(1) Two experiments, a preliminary and a main investigation, have been devoted to comparing right-left and center-alternate 'reports' with the usual left-right 'reports.' The former involved 25 *Ss*; the latter applied to 120 *Ss*. The 25

Ss were unselected; the 120 were selected in balanced groups on the basis of sex, handedness, and eyedness.

(2) The preliminary experiment contained expedients of questioned effectiveness to control fixation of the *Ss*' eyes. The main experiment employed 10 expedients to control fixation, which are believed to have proved their efficiency. The preliminary experiment failed; the main undertaking has, we believe, succeeded in a gratifying fashion.

(3) Among the 10 the chief expedients were two: (a) three variable types of card, presented in random and chance sequences, were (1) 'center-prints,' to obtain the effects of central fixation; (2) 'left-prints' to effect right-field fixations; and (3) 'right-prints,' to accomplish left-field fixations; and (b) three variable 'orders of report:' left-right (L-R), center-alternate (C-A), and right-left (R-L). Three additional expedients were the random sequences of 'prints,' random sequences of 'orders of report,' and a random sequence of combinations of 'prints' and 'orders of report.'

(4) Not only were R-L and C-A reports found possible of accomplishment, but they turned out to be approximately as efficient as the usual L-R orders of report.

(5) If there is a preferential and hard-to-break left-to-right tendency of 'recall,' or of 'report,' or of both, it failed to show itself in this investigation. Not only was 'recall' as an independent factor absent, or was translated into the 'report' influence, but the latter, the 'report' tendency, was itself lacking or was very weak.

(6) The prime condition to determine what letters will be 'reported,' in high frequency and accurately, was discovered to be the style of print, the type of fixation induced by the conditions listed under (3) above. All other factors, such as a tendency of L-R recall, proved to be merely incidental if present at all. Two much more important conditions than priority of recall or report have been discovered to be the eyedness and the handedness of the *Ss*, topics concerning which subsequent papers will attempt to shed some light.

COLOR PHENOMENA FROM ADJACENT RETINAL AREAS FOR DIFFERENT TEMPORAL PATTERNS OF INTERMITTENT WHITE LIGHT

By GLENN A. FRY, Washington University

In a previous paper[1] the writer has described certain color phenomena attending the intermittent exposure of a small bright white area. The effects of varying the brightness of the area, the frequency and duration of exposures, and the brightness of the surrounding area were investigated, and the results were shown to be compatible with Troland's suggestion that the transmission of color impressions from the retina to the cortex might depend upon the modulation of the optic nerve current.[2] In this paper a more complicated stimulus-situation is considered; namely, the stimulation of adjacent areas of the retina with different temporal patterns of intermittent white light.[3]

Obviously stimulus-patterns of this kind could be multiplied without number, but we may confine our attention to a few examples which illustrate the general underlying principles. This paper aims to show how the factors involved in a complex stimulus-situation can be analyzed, and how the facts can be accounted for on Troland's modulation theory.

If a given area of the retina, β, is stimulated intermittently and the surrounding part of the retina is also stimulated intermittently, that part of the field of vision corresponding to β may be seen colored and this color may arise in either of two ways: (1) the color may be induced upon β by intermittent stimulation of the surrounding area, a phenomenon related to that of color-contrast; (2) or it may depend upon the intermittent stimulation of β itself. Four criteria for the determination of the mode of production of the color are suggested by the following questions: (1) What is the effect of stimulating β steadily instead of intermittently? (2) What is the effect of stimulating the surrounding area steadily instead of intermittently? (3) What is the effect of leaving the surrounding area unstimulated? (4) What are the effects of varying the phase relations of the two sets of stimuli?

The color may be regarded as color-contrast induced upon β by intermittent stimulation of the surrounding area, (1) if the color can be similarly produced when β is stimulated steadily instead of intermittently, and (2) if the color can be produced only when the surrounding area is intermittently stimulated.

The color may be attributed to the intermittent stimulation of β, if the color cannot be produced when β is steadily stimulated instead of intermittently; and

* Accepted for publication June 14, 1933.

[1] G. A. Fry, Modulation of the optic nerve-current as a basis for color-vision, this JOURNAL, 45, 1933, 488-492.

[2] L. T. Troland, The enigma of color vision, *Amer. J. Physiol. Opt.*, 2, 1921, 43.

[3] The present paper, published by the author as National Research Fellow in the Biological Sciences, is from the Department of Ophthalmology, Oscar Johnson Institute, Washington University, St. Louis, Mo.

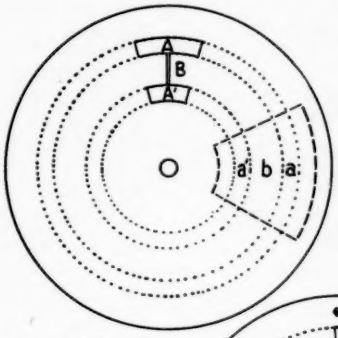


FIG. 1

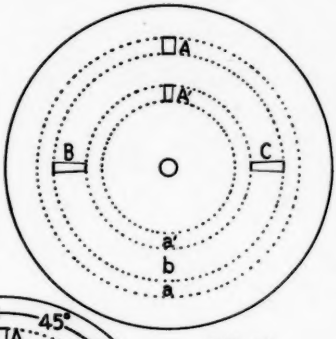


FIG. 2

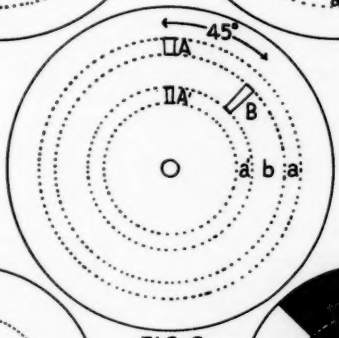


FIG. 3

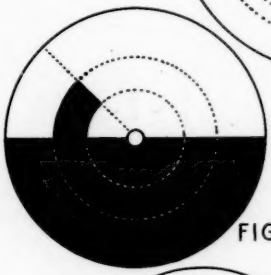


FIG. 4

FIG. 5

FIG. 6

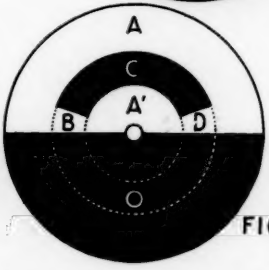


FIG. 7

FIGS. 1-7. SHOWING DETAILS IN THE CONSTRUCTION OF THE VARIOUS DISKS

TABLE I

ANALYSIS OF THE PHENOMENA OBTAINED WITH THE DISK IN FIG. 1

Character of Stimulation of β	Character of Stimulation of α and α'	Color of b	Color of a and a'
Intermittent	Intermittent	Green	Pink
	Steady*	Green	White
	Absent	White	Black
Intermittent	Intermittent	Green	Pink
Absent		Black	Pink

* If A and A′ are each divided into ten 3°-slits and these are evenly distributed around a and a', the frequency of stimulation for these zones is so rapid that it is equivalent to steady stimulation.

it need not be regarded as color-contrast induced by intermittent stimulation of the surrounding area, if the color can be produced when the surrounding area is either steadily stimulated or not stimulated at all. There are cases in which the color can be produced when the surrounding area is steadily stimulated, but cannot be produced if the surrounding area is left unstimulated.

The significance of the effects obtained by varying the phase relations of the two sets of stimuli can be understood better after a few concrete examples have been presented.

Let us illustrate the general principles stated above by applying them to a few concrete examples. The disks in Figs. 1, 2, and 3 are made of black cardboard and are 10 in. diam. In each case zone b is 1 in. wide; zones a and a' are each ½ in. wide; and zone d' is 2 in. from the center of the disk. A, A′, B, and C represent open slits. In the case of Fig. 1, A and A′ are 30° wide and B is 1° wide. In the case of Fig. 2, A, A′, B, and C are each 5° wide. In the case of Fig. 3, A, A′, and B are each 5° wide and the leading edges of A and A′ are 45° from the leading edge of B. These disks were rotated before a bright surface, which consisted in a ground glass illuminated from behind by two 250-watt lamps. The brightness of this surface was 402.7 c. per sq. ft. and its size and position with respect to the disks are indicated by the dash line in Fig. 1. The experiments were performed in a dark room. The observations were made at a distance of 12 ft.

For the sake of convenience the symbols, α, β, and α′, will be used to refer to the three zones of the retina corresponding to the three zones of the disk, a, b, and d' respectively.

When the disk in Fig. 1 is rotated 10 times per sec. before the bright surface, zone b is seen green and zones a and d' pink. An analysis of this phenomenon was made in accordance with the criteria mentioned above and the results are presented in Table I. The intermittent stimulation of α and α′ aids in the production of the green color of b, for the green cannot be produced when α and α′ are left unstimulated. However, the green need not be regarded as color-contrast induced upon β by intermittent stimulation of α and α′, for the green can be produced just as well when α and α′ are steadily stimulated instead of intermittently. Neither is the pink of zones a and d' color-contrast induced by

intermittent stimulation of β, for the pink is produced even when β is left unstimulated.

Fig. 8 illustrates a possible conception of what takes place in the three zones of the retina corresponding to the three zones of the disk. α and α′ depress activity of β[4] during the intervals between successive stimulations and thus help to modulate it. It may be supposed further that this modulation subserves the production of the sensation green. At least this inference is in accordance with Troland's suggestion that the transmission of color impressions from the retina to the cortex may depend upon the modulation of the currents in the various optic nerve-fibers. The activity of α and α′ seems to be capable of being modulated by its own intermittent stimulation without the aid of β.

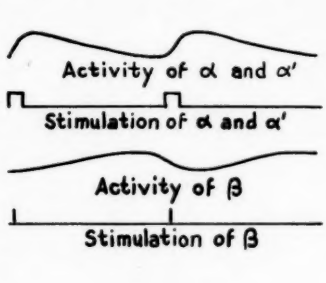


FIG. 8

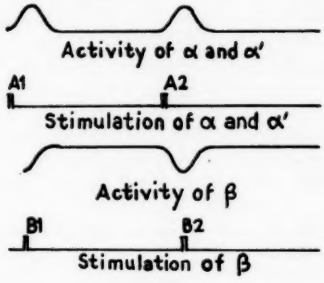


FIG. 9

FIGS. 8-9. ILLUSTRATION OF THE RETINAL PROCESSES

It must be supposed that varying the phase-relations of the two sets of modulation-waves in no way interferes with the wave-pattern of either set, because varying the phase of B with respect to A and A′ has no effect upon the colors.

In the case of the disk in Fig. 2 color-contrast undoubtedly comes into play. When the disk is rotated 10 times per sec. before the bright surface, zones *a* and *a′* are seen blue and zone *b* yellow. An analysis of this disk, similar to the analysis of the disk in Fig. 1, results in the data of Table II. The yellow of zone *b* must be regarded as color-contrast induced by intermittent stimulation of α and α′ for it cannot arise in any other way. The yellow can be produced when β is steadily stimulated so that it cannot be attributed to the intermittence of stimulation of β. If α and α′ are left unstimulated, *b* is not seen yellow but a flickering white. If α and α′ are steadily stimulated *b* is seen as light blue instead of yellow. In other words, *b* is seen as yellow only when α and α′ are intermittently stimulated.

Zone *b*, in turn, aids in the production of the blue of *a* and *a′*; the blue cannot be produced if β is left unstimulated, for under these conditions only a flickering white sensation is obtained. The blue is produced, however, when β is

[4] It is not necessary to assume that this depressive effect has its seat entirely in the retina. Interaction between the various retino-cortical pathways might take place not only at the retina but also at the basal ganglia and the cortex.

TABLE II

ANALYSIS OF THE PHENOMENA OBTAINED WITH THE DISK IN FIG. 2

Character of Stimulation of β	Character of Stimulation of α and α'	Color of b	Color of a and a'
Intermittent	Intermittent	Yellow	Blue
	Steady†	Bluish white	Yellow
	Absent	White	Black
Intermittent	Intermittent	Yellow	Blue
Steady*		Yellow	Blue
Absent		Black	White

* B and C were divided each into five 1° slits and these were evenly distributed around zone *b*. The frequency of stimulation for this zone was then so rapid that it was equivalent to steady stimulation.

† A and A′ were divided each into ten 1° slits and these were distributed evenly around zones *a* and *a′*, thus making the stimualtion of these zones equivalent to steady stimulation.

steadily stimulated as well as when β is intermittently stimulated. The blue is produced only when α and α′ are intermittently stimulated and is not produced if α and α′ are steadily stimulated.

A plausible account of what takes place in the three zones of the retina corresponding to the three zones of the disk is approximately as follows. The activity of zone β depresses the activity of α and α′ during the intervals between successive stimulations and thus helps to modulate it. However the activity of α and α′ does not have in return a similar effect upon the activity of β, for β acts as if it were steadily stimulated and shows no tendency to set up a rhythmic modulation of its own. The modulation which it acquires, and which subserves the yellow color-sensation, must be induced upon it by the rhythmic activity of α and α′. It might be supposed that the activity of β tends to wax and wane reciprocally with that of α and α′, being depressed when the activity of α and α′ waxes and enhanced when it wanes.

If the intermittent stimulation of zone β is equivalent to steady stimulation, we have an explanation of the fact that varying the phase of A and A′ with respect to B and C has no effect upon the colors.

The disk in Fig. 3 presents an entirely different situation. When it is rotated anticlockwise before the bright surface, zone *b* appears yellowish white and zones *a* and *a′* purplish blue. In spite of the fact that all the slits are the same size, the purplish blue appears much darker than the yellowish white. When the direction of rotation is reversed, the colors appear reversed.

Fig. 9 illustrates a possible conception of what takes place in the three zones of the retina corresponding to the three zones of the disk. Let us suppose that the disk is rotating anticlockwise with B following close on to A and A′. During the long periods from B1 to A2, and from B2 to A3 the activity of β depresses the activity of α and α′, and during the short periods from A2 to B2, and from A3 to B3, α and α′ depress β.[5] Thus the activity of β waxes and wanes re-

[5] The fact that *a* and *a′* appear darker than *b* shows that β depresses α and α′ more than they depress β.

ciprocally with that of α and α', and it might be supposed that the difference in color depends upon the difference in the shapes of the modulation-waves.

When adjacent areas of the retina are stimulated with different temporal patterns of intermittent white light, peculiar color phenomena are apt to arise at the juncture of the two areas. Thus when the disk in Fig. 4 is rotated anticlockwise 4 times per sec., illuminated with a 250-watt lamp placed 2 ft. away and observed at a distance of 2 ft., the band engendered by the moving arc appears to have a vivid green core and red borders. When the disk is rotated in the opposite direction, the band is seen to have dark blue borders and a purplish core and on each side of the band is a halo of very light blue. The fact that the colors at the border change with the direction of rotation indicates that peculiar physiological processes come into play.

Bidwell[6] pointed out that, when a white object is suddenly exposed in a dark field of vision, it is seen to have red borders for a moment. The essential conditions for the observation of Bidwell's red borders are provided by the disk in Fig. 5. When such a disk is rotated 4 times per sec., illuminated with a 250-watt lamp placed 2 ft. away and observed at a distance of 2 ft., the dark band is seen bordered internally on each side with red. These red borders are very narrow and much more difficult to observe than in the case of the disk in Fig. 4.

Bidwell's red borders owe their existence at least in part to chromatic aberration. Blue rays of light entering the eye come to a focus sooner than the red rays. Hence the retinal blur circle produced by a point source of white light must have either a blue core and a red border, or a red core and a blue border, depending upon whether the blue or the red rays come to a focus on the retina. Let us assume that at a distance of 2 ft. clearest vision is obtained when the blue rays come to a focus on the retina and the blur circles thus have blue cores and red borders. Accordingly the immediate outside of a white object should be seen red and the immediate inside blue. These colors are not seen when one gazes steadily at an object, but when a white object is intermittently exposed, as in the case of the disk in Fig. 5, red borders are actually seen. It is impossible to say why scattered red light is more effective under these conditions, but certain facts indicate that the actual stimulation of the retina with red light plays just as important a rôle as the intermittence of stimulation.

In the first place, the borders appear blue instead of red if a convex lens is placed in front of the eye so that the blue rays cannot come to a focus on the retina. The borders also appear blue if the observer stands 12 ft. away, provided A and A′ are very bright. It may be supposed that at this distance the blue rays cannot come to a focus. In the second place, if a prism is held before the eye, the border on one side appears blue and on the other red. Finally, it must be noted that Bidwell himself observed a suddenly exposed white object through ten different colored glasses and was able to see a red border only when red light was transmitted.

There is a fundamental difference between the disk in Fig. 4 and the disk in Fig. 5. If a prism is held before the eye when the disk in Fig. 4 is at rest, the border on one side of the black arc appears red and the border on the other

[6] S. Bidwell, On subjective colour phenomena attending sudden changes of illumination, *Proc. Roy. Soc.*, 60, 1896, 368-377.

blue; but when the disk is rotated anticlockwise, the blue border is apparently transformed into a purplish red one. However, in the case of the disk in Fig. 5, the blue border remains blue. The transformation of the blue into a purplish red in the case of the disk in Fig. 4 evinces the play of some peculiar physiological process.

Benham's disk, which is illustrated in Fig. 6, is of interest in this paper inasmuch as it involves the stimulation of adjacent areas of the retina with different temporal patterns of intermittent white light. When such a disk is rotated anticlockwise about 7 times per sec., and is illuminated with diffused daylight, the four groups of circles, engendered by the moving arcs, appear colored as follows: I, red; II, yellow; III, green; IV, blue. When the direction of rotation is reversed, the colors appear reversed.

All the previous investigators[7] seem to have arrived at the fundamental conclusion that the essential factor in determining the color of a given group of lines on Benham's disk is the time required for the disk to turn through the angular distance from the leading edge of the white sector to the given group of lines. The superficiality of this analysis may be demonstrated by the following experiment:

When a disk 8 in. in diam. like the one in Fig. 7 is rotated 8 times per sec., illuminated with a 250-watt lamp placed 1 ft. from the disk and at an angle of 45°, and observed at a distance of 12 ft., a flickering green band is seen. According to the conclusions of previous investigators the greenness of the band depends upon the position of the black arc, C, with respect to the leading edges of A and A′. Accordingly shifting the position of C ought to change the color. But if, as C is shifted, B, D, and O are shifted along with it without changing their sizes, the band still appears green no matter what position C assumes with respect to A and A′.

Instead of shifting B, D, and O along with C previous investigators kept the size and position of O constant and changed the relative sizes of B and D. In this way they obtained a difference in color and attributed it to the change in position of C, thus ignoring the change in the relative sizes of B and D as a possible determining factor.

When an analysis of Benham's disk is made in accordance with the principles outlined in this paper, the following conclusions may be reached: (1) The red of the first group of lines is a border phenomenon related to that obtained with the disk in Fig. 4 (rotated anticlockwise). (2) The yellow of the second group of lines is a color contrast effect induced by intermittent stimulation of the surrounding area and is similar to the yellow obtained with the disk in Fig. 2. (3) The green of the third group of lines is produced by intermittent stimulation and is analogous to the green obtained with the disk in Fig. 7. (4) The blue of the fourth group of lines is allied to what is obtained with the disk in Fig. 4 (rotated clockwise) but it has not been determined to what extent it is a core phenomenon and to what extent a border phenomenon.

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[7] An account of the various investigations of Benham's disk may be found in an article by H. Piéron, Le mécanisme d'apparition des couleurs subjectives de Fechner-Benham *L'année psychol.* 23, 1922, 1-49.

Summary

(1) The color phenomena attending stimulation of adjacent areas of the retina with different temporal patterns of intermittent white light have been investigated. The factors involved in a complex stimulus-situation of this kind can be analyzed, and a few examples have been presented to illustrate the general underlying principles. The facts have also been shown to be compatible with Troland's suggestion that the transmission of color impressions from the retina to the cortex may depend upon the modulation of the optic nerve-current.

(2) When adjacent areas of the retina are stimulated with different temporal patterns of intermittent white light, color phenomena are apt to arise at the juncture of the two areas. These border phenomena are difficult to understand because they depend upon complex stimulus-conditions which involve chromatic aberration. Other puzzling physiological processes appear also to come into play.

(3) An attempt has been made to show wherein previous investigators have failed to discover the principles underlying Benham's disk, a complicated stimulus-pattern which involves the stimulation of adjacent areas of the retina with different temporal patterns of intermittent white light.

THE RESPONSE OF WARM SPOTS UNDER SUCCESSIVE STIMULATION

By RICHARD H. EARHART, Columbia University, and
KARL M. DALLENBACH, Cornell University

Careful investigators of the temperature senses have, in order to avoid the effects of fatigue and 'tuning,' repeated the stimulations of a given spot on the skin only at long temporal intervals. Such a procedure not only prolongs the duration of the experiments, but gives rise to technical difficulties[1] that are almost as grievous as the effects it is supposed to avoid.

To determine whether the long delays between successive stimulations are necessary for accurate results with cold spots, Drury and Dallenbach repeatedly stimulated a number of such spots at intervals of 5, 15, 20 sec. and 1, 3, 5, 20 min.[2] They found that "tradition . . . has needlessly erred upon the side of safety;"[3] that stimulations may safely be made at 20-min. intervals, and almost as safely—provided only a few repetitions are made—at intervals of 1, 3, and 5 min.; for "it is not the rate as much as the frequency of stimulation that conditions the response of the cold spots."[4]

Since it would be unwarranted to assume that the results obtained with cold spots would hold for warm spots, the present study was undertaken. We seek here to do for the warm spots what Drury and Dallenbach have done for the cold.

METHOD

Apparatus and procedure. A procedure similar to that employed in the study of cold spots was used in the present investigation. An area, 5 x 10 cm., was selected on the dorsal side of *O*'s right upper forearm. The area was shaved at the beginning of the experiment so as to avoid distractions which might result from chance stimulation of the hairs. During stimulation, *O*'s arm was held firmly and comfortably in position by means of a plaster of Paris arm-rest cast especially for him.

A Cornell thermo-aesthesiometer[5] was brought to the desired temperature by means of a stream of warm water forced through it by the pressure of the University mains. The stream, before coming to the aesthesiometer, passed through a copper coil immersed in a bath of hot water. The desired temperature of the stimulus-point was obtained by varying the water's rate of flow through the apparatus; it then remained constant within the limits of 42°—43° C.[6]

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* Accepted for publication August 20, 1932. From the Psychological Laboratory of Columbia University.

[1] For a discussion of these see K. M. Dallenbach, The temperature spots and end-organs, this JOURNAL, 39, 1927, 409-413.

[2] M. B. Drury and K. M. Dallenbach, The response of cold spots under successive stimulation, *ibid.*, 43, 1931, 490.

[3] *Op. cit.*, 496.

[4] *Ibid.*

[5] Dallenbach, Some new apparatus, *ibid.*, 34, 1923, 92 ff.

[6] The temperature of the stimulus-point was calculated as follows. The stream of warm water was divided by means of a Y-tube after it had passed through the bath. One branch led to the aesthesiometer; the other, passing through an equal

At the beginning of every experimental period 10 warm sports were located and marked with ink. After a 20-min. rest, the spots were each restimulated 11 times over at one of the following time-intervals: 5, 15, or 30 sec., or 1, 3, or 5 min. The shorter intervals were used first, and only one time-interval was used during an experimental period.

Pressure of stimulation was kept constant by means of the spiral spring and gauge on the aesthesiometer, and rate by means of a metronome set to beat seconds. For all intervals except the 15-sec., stimulation followed a 6-fold count. *E*, the junior author, counted "one, two, three, four, five, six," silently in rhythm with the metronome and applied the stimulator at "one," removed it at "two," held it off the skin during the counts of "three, four, five, six"—while *O* gave his report[7]—and then applied it again on the count of "one," and so on until the completion of the series. With the 5-sec. interval, the spots were taken singly and stimulated in rhythm with this count 11 times over. Rests were permitted, when requested by *O*, between series with different spots. With the 30-sec. interval, the spots were taken in groups of 5, and stimulated in rotation and in rhythm with the 6-fold count. A rest was given *O* between the two groups of 5. With the longer intervals of 1, 3, and 5 min., all 10 of the marked spots were stimulated in rotation and in rhythm with the 6-fold count. The spots were also taken singly with the 15-sec. interval, but the stimulation was changed to the rhythm of a 15-fold count.

In all the series, excepting those with the 30-sec. and 1-min. intervals,[8] unmarked spots were stimulated, without *O*'s knowledge, in rhythm with the marked spots either before or after the series. Since the unmarked spots were chosen haphazardly, some were sensitive to warmth and some were not. Our object in introducing them was two-fold: we did not wish *O* to become habituated in his report, and we thought, by stimulating warm spots that had not before been stimulated, to provide him with a ready means of gauging his intensive judgments.

Since the first restimulation of every spot was made approximately 20 min. after its localization, the first responses—60 in all, *i.e.* 10 for every one of the 6 different time-intervals—were treated as results of a 20-min. stimulation-interval.[9] The 10 remaining restimulations gave a total of 100 responses for every one of the 6 different temporal intervals—600 responses for every *O*.

The experiment was performed during July, 1932. The temperature of the laboratory varied between 29°–34° C.

Observers. The *O*s were M. P. Crawford (*C*) and M. Koch (*K*), graduate students in psychology, and the senior author (*D*). *C* and *K* came to the experiment

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length of rubber tubing, to a thermometer. Since the two branches were of equal length, and delivered equal amounts of water, the temperature of one branch was assumed to be the same as that of the other. A more precise determination was unnecessary as our principal object was to keep the temperature relatively constant and below the limen for heat.

[7] These reports were recorded by a second *E*. The sequence of the stimulations was too rapid to permit a single *E* doing this work. Our thanks are due Mr. T. A. Jackson and Mr. J. Foley for serving as the second *E*.

[8] The sequence of the stimulations with these intervals was continuous, as soon as one rotation was made another was begun, consequently no checks could be introduced.

[9] This time is approximate for the longer stimulation-intervals, but for the shorter, particularly the 5- and 15-sec. intervals, the time varied from 20 to 40 min.

untrained in the observation of warmth; preliminary practice, however, familiarized them with the quality of warmth and its intensive gradations.

Instructions. Instructions, similar to those used by Drury and Dallenbach,[10] were read to the *O*s at the beginning of every experimental hour. They were as follows:

"You will be stimulated with a warm point. Report when you experience warmth, and then describe the intensity of your experience in terms of the following four point scale: intense, moderate, mild, and weak."

Results

The general results of the study are given in Table I, which shows for every *O* the number of times the warm spots responded at every time-interval. For all the *O*s and all the intervals there was a high percentage of responses: from 87–100% for *C*, 88–100% for *D*, and 82–100% for *K*. There is, however, no agreement among the *O*s on the most or least favored interval. The highest percentage—if we omit from present

TABLE I

Showing, for Every Interval, the Number of Times Warmth Was Reported and the Intensity-Level of the Reports

O	Interval	Intensity level				Total
		Intense	Moderate	Mild	Weak	
	5 sec	14	42	29	2	87
	15 sec	13	47	22	11	93
	30 sec	2	26	38	25	91
C	1 min	6	32	26	23	87
	3 min	12	48	23	15	98
	5 min	13	45	24	16	98
	20 min*	23	55	17	5	100
	5 sec	30	38	15	10	93
	15 sec	6	27	42	13	88
	30 sec	24	35	26	11	96
D	1 min	18	39	38	5	100
	3 min	9	32	37	20	98
	5 min	9	28	43	19	99
	20 min*	42	33	23	2	100
	5 sec	9	38	20	15	82
	15 sec	24	45	18	11	98
	30 sec	27	39	22	11	99
K	1 min	40	34	11	6	91
	3 min	44	36	11	9	100
	5 min	28	37	17	15	97
	20 min*	25	48	17	10	100

* But 60 stimulations were given at this interval. The values here are expressed in ratios of 100 so that they would be comparable to those at the other intervals.

consideration the 20-min. interval where conditions differed from those in the other intervals—was obtained at the intervals of 3 and 5 min. for *C*, of 1 min. for *D*, and of 3 min. for *K*; the lowest percentage, at intervals of 5 sec. and 1 min. for *C*, of 15 sec. for *D*, and of 5 sec. for *K*.

[10] *Op. cit.*, 491.

Table II groups the data into two classes of intervals: 'long' (1, 3, and 5 min.), and 'short' (5, 15, and 30 sec.). As is shown in this table, more reports of warmth were returned by all the *O*s at the 'long' intervals than at the 'short;' 283 to 271 by *C*, 297 to 277 by *D*, and 288 to 279 by *K*. The averages for the contrasted intervals, the differences, and the reliability of those differences are given in Table III. The

TABLE II

SHOWING, FOR THE CONTRASTED INTERVALS, THE NUMBER OF TIMES WARMTH WAS REPORTED AND THE INTENSITY-LEVEL OF THE REPORTS

(The 5-,15-,and 30-sec. intervals are classified as short; the 1-,3-,and 5-min. intervals as long.)

O	Interval	Intensity level				Total
		Intense	Moderate	Mild	Weak	
C	long	31	125	73	54	283
	short	29	115	89	38	271
D	long	36	99	118	44	297
	short	60	100	83	34	277
K	long	112	107	39	30	288
	short	60	122	60	37	279

TABLE III

SHOWING, FOR THE CONTRASTED INTERVALS, THE AVERAGE NUMBER OF TIMES WARMTH WAS REPORTED, AND THE SIGNIFICANCE OF THE DIFFERENCES BETWEEN THE AVERAGES

O	Long		Short		D	σ diff.	D/σ diff.
	Av.	σ	Av.	σ			
C	94.33	1.33	90.33	1.71	4.00	2.17	1.84
D	99.00	0.18	92.33	1.54	6.67	1.64	4.07
K	96.00	1.13	93.00	1.47	3.00	1.86	1.61

results are statistically reliable for only one (*D*) of our *O*s. *D*'s difference in favor of the 'long' intervals is 6.67 and his D/σ diff. is 4.07; *i.e.* 99.9+ times in 100, the difference would lie in the same direction. The differences for all the *O*s, however, lie in the same direction and are consequently, when considered together, highly reliable. The conclusion, therefore, seems warranted that the longer temporal intervals are more favorable than the shorter for the stimulation of warm spots. This conclusion is also borne out by the results of the 20-min. interval (see Table I), as all the *O*s report warmth in 100% of these stimulations.

Not only are more reports of warmth returned with the longer intervals, but higher intensities are also in general reported at those intervals. More 'intense' and 'moderate' reports are returned by all the *O*s after the 20-min. interval than with any other (see Table I), and a similar relationship appears in the reports of *C* and *K* after the other intervals (see Table II). Higher intensities occur more frequently after the 'long' than after the 'short' intervals, and the lower intensities more frequently after the 'short' than after the 'long.' *D*, on the contrary, and also in opposition to his own results with the 20-min. intervals, reports higher intensities more frequently after the 'short' intervals and lower intensities more frequently after the 'long.'

Both of these facts may be readily explained, though the explanations, as do the

results, stand in opposition. The fact that the longer intervals give higher intensities may be explained upon the basis of fatigue and recovery: stimulation reduces sensitivity by way of fatigue; recovery, however, sets in immediately and progresses with time; consequently, the longer the interval between successive stimulations, the greater the recovery and the greater the intensity of the subsequent response. The fact that the shorter intervals give higher intensities may be equally well explained, on the other hand, by 'tuning:' stimulation 'tunes' the spot and makes it respond more intensely; the effect of 'tuning' wears off with time; consequently, the shorter the interval between successive stimulations, the greater the effect of 'tuning' and the greater the intensity of the subsequent response. Not much is known about the effects of 'tuning,' and our results contribute little; they set a problem rather than offer anything toward its solution.

The effect of repetition is shown in Table IV, which gives for the 'long' and the 'short' intervals the number of times the spots failed to respond to every one of the

TABLE IV

SHOWING, FOR THE CONTRASTED INTERVALS, THE NUMBER OF TIMES WARMTH WAS NOT REPORTED AT EVERY ONE OF THE 11 SUCCESSIVE STIMULATIONS

O	Interval	Stimulations											Total
		1	2	3	4	5	6	7	8	9	10	11	
C	long	0	0	0	5	0	1	2	1	2	2	4	17
	short	0	2	0	2	2	4	4	4	4	2	5	29
D	long	0	0	0	0	0	0	0	0	0	1	2	3
	short	0	1	3	1	2	2	1	2	3	3	5	23
K	long	0	1	1	1	1	1	3	1	0	3	0	12
	short	0	3	1	5	2	2	1	3	2	0	2	21

11 stimulations. The 'failures' increase for *C* and *D* with frequency of stimulation, but for *K* they remain practically constant. The cases, however, are few in number, so that this relation does not become clear until the 'failures' of the first stimulations are set against those of the later. Omitting the first stimulation, as it was made after a 20-min. rest period, the total number of 'failures' for the next 5 stimulations (*i.e.* stimulations 2–6) was 16, 9, and 18 for *C*, *D*, and *K* respectively, and for the last 5 stimulations (*i.e.* stimulations 7–11) 30, 17, and 15 respectively. That is, almost twice as many stimulations failed to arouse warmth in the case of *C* and *D* during the last half of the experiment as during the first half. *K*, on the contrary, reported more failures during the first half, 18 as opposed to 15. *C*'s and *D*'s results may be explained by fatigue, and *K*'s results by 'tuning.' But here again results as well as explanations are conflicting, and, adding to the perplexity, *D* and *K* have changed places, so that we are denied the refuge of "individual differences."

Table V classifies the 'failures' for each of the contrasted intervals upon the basis of their early or late occurrence. With the 'long' intervals, *C* 'failed' in 6 of the early stimulations and in 11 of the later, *D* in 0 and 3, and *K* in 5 and 7. *C* and *D* exhibit the same relationship—but with smaller differences—in the 'short' intervals; but *K* shows a reversal, with 13 'failures' in the early stimulations as against 8 in the later. Whatever may be the ultimate explanation of these results, they are uniform

TABLE V

SHOWING, FOR THE CONTRASTED INTERVALS, THE NUMBER OF TIMES WARMTH WAS NOT REPORTED DURING THE EARLY AND THE LATE STIMULATIONS

Interval	Stimulation	Observer		
		C	*D*	*K*
long	2–6	6	0	5
	7–11	11	3	7
short	2–6	10	9	13
	7–11	19	14	8

for the *O*s in the 'long' intervals and they indicate once more that the 'long' intervals are preferable to the 'short.'

The effect of repetition is further shown in the different intensities reported during the early and the late stimulations. In order that these differences may be set in sharp contrast, only the intensive reports during the first and last 3 stimulations at all the time-intervals are given in Table VI. This table shows that more 'intense' and

TABLE VI

SHOWING THE ANALYSIS OF THE REPORTS OF THE FIRST 3 AND LAST 3 STIMULATIONS

O	Stim. No.	Intensity				No Rep.	Stim. No.	Intensity				No Rep.
		I	Mo	Mi	W			I	Mo	Mi	W	
C	1	14	33	10	3	0	9	7	20	18	9	6
	2	8	31	13	6	2	10	6	17	19	14	4
	3	5	29	15	11	0	11	5	23	14	9	9
	Total	27	93	38	20	2	Total	18	60	51	32	19
D	1	25	20	14	1	0	9	9	14	26	8	3
	2	20	24	12	3	1	10	5	12	24	15	4
	3	16	23	16	2	3	11	3	16	21	13	7
	Total	61	67	42	6	4	Total	17	42	71	36	14
K	1	15	29	10	6	0	9	20	18	10	10	2
	2	19	22	7	8	4	10	16	27	9	5	3
	3	12	36	6	4	2	11	14	25	11	18	2
	Total	46	87	23	18	6	Total	50	70	30	23	7

'moderate' warmths and fewer 'mild' and 'weak' ones were reported by all the *O*s during stimulations 1–3 than during 9–11. Grouping the intensive extremes, we find that *C* reported the upper two levels during the first and last 3 stimulations 120 and 98 times respectively, *D* 128 and 59, and *K* 133 and 120. The two lower intensive levels, similarly, were reported 58 and 83 times by *C*, 48 and 107 times by *D*, and 41 and 53 times by *K*. The greater effectiveness of the first three stimulations over the last three is also shown by the 'failures.' Fewer 'failures' were reported by all the *O*s during stimulations 1–3 than during 9–11. These data indicate that the number of stimulations that a spot has undergone determines to a great extent the nature of the subsequent responses. Since this point obtrudes even when all the temporal intervals are combined, we are of the opinion that, within the limits of our experiment, frequency of stimulation is of greater weight than rate.

In order to determine the consistency of the responses of the various spots and the correlation of the later responses with the first response, every one of the warm spots was characterized, on the basis of the first report, as intense, moderate, mild, or weak.[11] The responses during the subsequent stimulations showed an intensive decline, but the number of responses and the extent of the intensive decline was closely correlated with the intensity of the first response. The greater the intensity of the first response, the greater were the intensities subsequently reported; and, contrariwise, the weaker the intensity of the first response, the weaker were the intensities reported later. This correlation is also confirmed by the 'failures.' Fewer 'intense' spots, for all the *O*s, failed to respond to restimulation than 'moderate,' fewer 'moderate' than 'mild,' and fewer 'mild' than 'weak.' These results indicate that the 'spot' as well as rate and frequency of stimulation conditions the response. If only those spots reported as 'intense' upon the first stimulation are selected for experimental purposes, less attention and care need be given to the rate of stimulation.

Summary and Conclusions

The greatest percentage of reports of warmth was obtained from all the *O*s after a 20-min. interval (Tables I and II). There was no agreement as to the other intervals (5, 15, 30 sec., or 1, 3, or 5 min.). More reports, however, were returned by all the *O*s with the 3 long than with the 3 short intervals. Though the differences between the 'long' and 'short' intervals were small—being statistically reliable for but one *O*—they were in the same direction for all the *O*s.

Intensity of response parallels frequency (Tables I and II). Higher intensities were more frequently reported after the 20-min. interval than after any other, but beyond that there was again no agreement among the *O*s. Two of these reported more high intensities after the 'long' intervals, and more low intensities after the 'short.' One *O*, on the contrary, reported more high intensities after the 'short' intervals.

Both frequency and intensity of response correlate inversely with frequency of stimulation (Tables IV–VI). For every interval and every *O*, the greater the number of stimulations the fewer relatively the number of reports and the lower relatively the level of intensity reported. The differences among the intervals in frequency and intensity of response is smallest during the early stimulations and greatest during the late ones (Tables V and VI). These results suggest that frequency of stimulation is of greater significance in determining both frequency and intensity of response than the rate of stimulation, *i.e.* the time-interval between successive stimulations.

There is a high correlation between the intensity of the first response of a spot and the intensity of the later responses. Spots characterized at their first stimulation as 'intense' later give fewer 'failures,' more responses of high intensity, and fewer of low intensity than spots first characterized as 'moderate,' and similarly for the 'moderate' in comparison with the 'mild,' and the 'mild' in comparison with the 'weak.' Frequency and intensity of response seem to depend more upon the specific spot stimulated than upon either rate or frequency of stimulation.

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[11] Upon this basis *C* had 14 intense, 33 moderate, 10 mild, and 3 weak spots; *D*, 24 intense, 20 moderate, 15 mild, and 1 weak; and *K*, 17 intense, 29 moderate, 10 mild, and 4 weak.

Upon the basis of these results we feel justified in concluding that the response of a warm spot in a series of stimulations depends upon the kind of spot selected and upon the frequency and rate of stimulation. Unless the experiment is directed upon intensive differences, intense spots should be selected for stimulation as they are the most persistent and consistent in their response. If such spots are selected and if they are to be restimulated only a few times, the rate is relatively unimportant; but if the spots are unselected, or if the number of restimulations is to be large, the rate becomes increasingly important.

Intervals of 20 min. are adequate for all spots and for any number of restimulations. Shorter intervals, however, may be safely used if intense spots are selected or if only a few restimulations are to be made. If either of these conditions is satisfied, intervals of from 1 to 5 min. are sufficiently long for reliable and consistent results; if both conditions are satisfied, intervals of from 15 to 30 sec. may safely be used. The slightly greater consistency in favor of the longer intervals is more than made up by the higher accuracy in locating the same spot after a briefer interval.

The results of this study agree closely with those obtained by Drury and Dallenbach for cold spots. Temporal conditions favorable for the arousal of cold are also favorable for the arousal of warmth. Whatever difference, however, exists between the results of the two studies lies in favor of the warm spots. A greater percentage of warm spots responded upon successive stimulation than of cold spots, and the responses of the warm spots were more evenly distributed over the intensive range.[12] These results stand in opposition to general opinion, but they are in line with Dallenbach's findings in an earlier study of the temperature spots. He found "that the warm spots were more easily and more definitely identified than the cold,"[13] and concluded that "it is only an observer who is naïve in the introspection of the qualities of the temperature spots, who describes his warm experiences as 'indefinite,' 'vague,' 'illy defined,' 'less insistent,' 'more difficult to localize than the cold'."[14] With this statement, the present investigation is in substantial agreement.

[12] A comparison of the data of the two studies reveals the following relations: 'intense' responses, 328 warm against 123 cold; 'moderate,' 668 warm against 610 cold; 'mild,' 462 warm against 674 cold; and 'weak,' 237 warm against 47 cold.

[13] K. M. Dallenbach, The temperature spots and end-organs, this JOURNAL, 39, 1927, 423 f.

[14] *Ibid.*, 424.

THE MEANINGFULNESS OF 320 SELECTED NONSENSE SYLLABLES

By CLARK L. HULL, Yale University

Most experimental studies of learning and memory in which nonsense syllables are employed are quantitative in their nature. This fact makes the relative ease of learning the individual nonsense syllables a matter of prime importance. So far as the writer is aware, no direct evidence as to the relative memorization difficulty of nonsense syllables has been published.[1] It is through its indirect bearing on this important question that the relative meaningfulness of the various nonsense syllables possesses significance, the *a priori* expectation being that the more meaningful syllables would be more easily memorized.

It requires but a glance through an ordinary set of nonsense syllables to make it clear that many such syllables do not conform strictly to the implied criterion of being without meaning. This result of casual observation has been substantiated experimentally by Glaze, who determined the association value of over two thousand nonsense syllables.[2] Glaze presented his syllables by means of a tachistoscope with a maximum exposure interval of 3 sec. At the time of presentation the experimenter also spelled each syllable. The 16 *S*s were asked to report anything the syllable made them think of. While, under these conditions, a considerable number of syllables were found to have zero association values, *i.e.* to be in some sense strictly nonsense, a surprisingly large number of other syllables were found to have 100% association value. An inspection of this latter list caused the writer to wonder whether Glaze's findings might not be in part the result of the particular technique employed in the investigation.

In order to test this hypothesis, a sample of 320 syllables was chosen for special investigation largely on the grounds that they appeared to be easy to pronounce and that 256 of them appeared to be relatively without meaning. In testing these syllables for meaningfulness it was desired to secure the evidence under conditions as closely as possible resembling those of actual learning. Accordingly, the material was presented visually by means of the writer's automatic memory apparatus, as in a regular memory experiment. The syllables were printed on light buff paper in black capital letters by a regular printing press. The letters were .25 in. high (18 pt). The window of the apparatus was 1 in. wide and 1.1 in. high. The syllables as they appeared at the window were well lighted by a desk lamp, so shaded that the direct rays from the lamp did not enter the *S*'s eyes. They were exposed in series of 16 syllables, each

* Accepted for publication June 17, 1933. Miss Ruth Hays acted as laboratory technician throughout this investigation.

[1] It would not be difficult to set up an experiment which would yield such results, though its execution would be laborious. The major constant errors besetting such an experiment would be the progressive effects of practice on the *S*'s capacity to learn, and the greater difficulty of memorizing syllables which fall near the middle of rote series. These respective difficulties could be avoided by systematically rotating the series from *S* to *S*, and by systematically rotating the syllables within each series.

[2] J. A. Glaze, The association value of non-sense syllables, *Ped. Sem.*, 35, 1928, 255-269.

syllable being in clear view for 2 sec. The 20 series were rotated among the *S*s in such a way that each series appeared equally often in every stage of practice.

In order further to simulate the conditions of a memory experiment, each series of 16 syllables was exposed 3 times in succession, and the *S* was instructed to try to memorize them. The following instructions were presented in typewritten form:

This is an experiment in learning a list of nonsense syllables, and not a psychological test. We are interested in certain complex relationships of the learning process common to all people, and not at all concerned with your personal reactions.

Shortly after the apparatus starts you will see a 3-letter syllable in the window. You are to pronounce this syllable and those that follow it as you see them. After you have seen the list once, you are to endeavor to anticipate the syllables; in other words, as you see one syllable you are to pronounce the syllable that will follow it *before* it appears. Also, as each syllable appears, if it happens to make you think of anything in particular, tell me as briefly as possible what it is. Please do not *try* particularly to think of associations for the various syllables, but if you do think of one, be *sure* to tell me of it.

In a few cases where the *S*s seemed to be disregarding the instructions, the relevant portions were called to their attention orally.

Twenty male university students were employed as *S*s. These were selected to the extent that graduate students majoring in foreign languages were avoided. None had ever taken part in a memory experiment before. They were paid by the hour for their services.

Accurate record was made by the *E* of all reactions made by *S* on each of the 3 presentations of each syllable. It was found, in general, that the reports of meaningfulness increased with successive presentations. If a syllable was reacted to as meaningful at the first presentation, it would usually be reacted to at the two remaining ones, though there were frequent exceptions to this rule. In general there was an increase in meaningfulness at each successive presentation; for the 320 syllables as a whole, the first presentation of the syllables evoked 1040 reactions, the second 1391, and the third 1492. Usually, also, if a man gave a particular reaction at one presentation of a syllable, he would give the same reaction at a subsequent presentation, though two of the *S*s frequently gave different reactions to the same syllable. Glaze's finding that individuals differ enormously from one another as to the particular meaning which a syllable has for them, or whether it has any meaning at all was also confirmed.

In scoring the syllables for meaningfulness, it was assumed that a syllable which was reacted to on all three occasions was more meaningful than one reacted to twice, and one reacted to twice was more meaningful than one reacted to only once. Accordingly, each reaction made by an *S* to a given syllable was counted like every other, the maximum score a given syllable could receive being 60; *i.e.* 3 positive reactions by each of 20 *S*s. The percentage of meaningfulness was computed on this basis. Thus if a given syllable received a total of 24 reactions, its percentage of meaningfulness by this procedure would be 40. The percentage indices of meaningfulness for the 320 syllables are shown in Table I.

One answer to the primary question of the present investigation is found by comparing the distribution of percentage indices as shown in Table I with the values for the corresponding syllables as they appear in Glaze's report. Except for 11 syllables not found in Glaze's report and one (KIV) which appears in his table in two places, the two distributions are shown graphically in Fig. 1. There it may be seen that the present technique yields, upon the whole, much smaller values. The mean value by

TABLE I

ALPHABETICAL LIST OF 320 NONSENSE SYLLABLES WITH THE PERCENTAGE OF EXPOSURES AT WHICH A GROUP OF 20 Ss REPORTED MEANING

BAF	62	HAB	62	LAR	42	PEM	28	VED	23	YIL	12
BAP	33	HAJ	0	LEB	37	PIB	12	VEF	0	YIM	17
BAV	48	HAN	43	LEM	35	PIJ	3	VEK	20	YIN	18
BAW	57	HAP	73	LIJ	17	PIM	12	VEN	28	YIS	63
BEJ	5	HEB	33	LOY	50	PIV	28	VEP	5	YOB	10
BEP	10	HEF	40	LOZ	38	POB	2	VES	42	YOD	18
BEV	50	HIB	28	LUB	45	POF	15	VIB	8	YOJ	0
BEW	20	HIF	13	LUJ	12	POV	28	VIF	12	YOL	8
BIP	18	HIJ	3			POY	25	VIP	5	YOM	32
BIV	25	HUJ	0	MAF	0	PUD	48	VOD	23	YOS	33
BIW	2	HUV	12	MAV	27	PUM	52	VOF	0	YUF	2
BOF	12	HUX	57	MEB	17	PUV	17	VOK	8	YUK	8
BOJ	17			MEF	20	PUY	12	VOL	53	YUN	20
BOP	25	JAT	12	MEJ	13			VOM	18	YUT	5
BOV	45	JEB	8	MEP	8	RAF	37	VUD	12	YUX	7
BUP	12	JEK	20	MEV	3	REB	48	VUK	0		
BUV	12	JEX	8	MEZ	27	REJ	45	VUM	8	ZAH	8
BUW	5	JEZ	25	MIB	7	REZ	37	VUP	7	ZAK	13
		JID	3	MIP	12	RIK	28	VUR	13	ZAL	7
DAF	47	JIH	3	MIV	0	RIX	13	VUS	22	ZAM	17
DAJ	18	JIK	15	MOF	17	ROF	32	VUT	15	ZAN	15
DAK	32	JIS	12	MOJ	7	ROJ	25			ZAW	30
DAP	62	JIX	25	MOY	12	RUP	47	WAB	18	ZAX	22
DAX	12	JIY	7	MUB	15	RUV	15	WEF	12	ZEB	20
DEJ	7	JOD	15	MUJ	5	RUY	15	WEM	15	ZED	18
DEV	48	JOF	27	MUP	15			WES	55	ZEN	33
DEZ	35	JOM	17	MUX	20	TAH	10	WIB	5	ZER	12
DIB	18	JOP	20	MUY	18	TAJ	40	WIM	55	ZET	13
DIJ	7	JUB	22			TAV	43	WOB	7	ZEX	32
DOB	23	JUF	12	NAD	12	TAY	42	WOF	2	ZIB	3
DOY	18	JUP	25	NAF	3	TAZ	23	WOJ	5	ZID	7
DUJ	7	JUR	62	NAJ	12	TEB	12	WOL	35	ZIH	5
DUR	45			NAR	48	TEF	17	WOM	48	ZIK	5
DUT	50	KEB	12	NAS	37	TEP	20	WOX	20	ZIL	10
DUY	15	KEF	2	NAX	20	TEV	3	WOY	13	ZIR	15
		KEJ	0	NEF	27	TIB	28	WUB	17	ZIT	20
		KEM	32	NEJ	15	TIJ	7	WUK	17	ZIV	17
FAJ	8	KES	20	NEM	23	TIV	5	WUM	22	ZIX	27
FAP	5	KET	52	NEP	43	TOB	35	WUP	8	ZOD	30
FAV	37	KEX	7	NEV	53	TOJ	15	WUT	18	ZOF	8
FAW	28	KIB	22	NIJ	7	TOV	7	WUX	18	ZOH	13
FEK	15	KIF	10	NIM	17	TUD	15			ZOJ	0
FEP	0	KIH	5	NIV	18	TUJ	2			ZOL	18
FEX	28	KIJ	12	NOF	10	TUP	27	YAD	15	ZOM	15
FID	43	KIP	48	NOJ	13	TUS	42	YAF	12	ZOP	7
FIK	33	KIR	17	NOY	47			YAN	27	ZOR	18
FIM	32	KIV	18	NUP	30	VAB	2	YAR	33	ZOT	20
FIP	12	KIY	22	NUV	7	VAD	12	YAS	40	ZOX	33
FOD	42	KIZ	15	NUY	13	VAH	10	YAT	40	ZOY	5
FOJ	7	KOH	33			VAM	30	YAX	15	ZUD	3
FOS	43	KOJ	5	PAB	28	VAK	5	YED	17	ZUK	5
FOT	30	KOS	38	PAF	15	VAP	52	YEK	8	ZUL	27
FOV	5	KUJ	5	PAM	28	VAR	47	YEM	13	ZUN	25
FOW	38	KUN	13	PEB	42	VAW	3	YEV	7	ZUR	20
FUB	12			PEF	17	VAX	10	YIB	7	ZUT	12
FUP	2	LAJ	7	PEJ	2	VAY	17	YIK	7	ZUX	3

Glaze's method is 42.7, whereas that by the present method is 21.3, or almost exactly half as great. The σ of Glaze's distribution amounts to 25.7, whereas that by the present method amounts to 15.1. The difference between the means of the two sets of indices is 24.6 times its probable error.

A further question arises as to whether the values for syllables high in one series will also be high in the other, and vice versa. The product moments correlation coefficient between the scores obtained by the respective methods was found to be 0.48. On the surface this indicates a remarkable lack of agreement between the results by the two methods. In order to determine whether this value was seriously attenuated by inadequacies of measurement so far as the present technique is concerned,

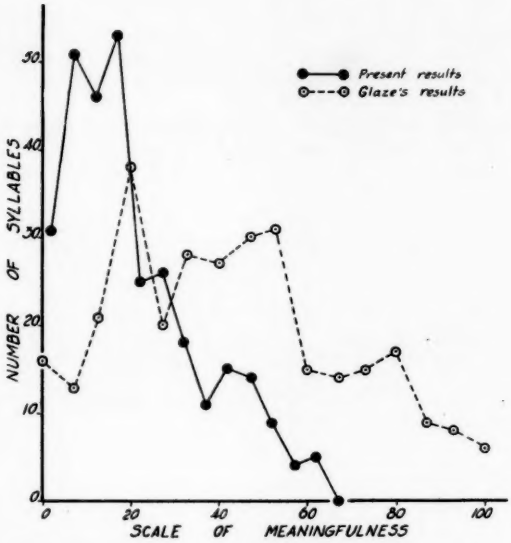

FIG. 1. DISTRIBUTATION OF 308 NONSENSE SYLLABLES WITH RESPECT TO MEANINGFULNESS AS DETERMINED BY GLAZE'S AND OUR TECHNIQUE
Note the relatively low degree of meaningfulness by the present procedure.

independent percentage indices were computed for each syllable on the basis of the results obtained from the first 10 *Ss* run and those obtained from the second 10. The correlation between these amounted to 0.64. When the Spearman-Brown formula is applied to this value, it appears that the scores from the entire 20 *Ss* would probably correlate with scores obtainable from 20 other *Ss* similarly chosen, to the extent of 0.78. If it be assumed as a first approximation that the proportionate reliability of Glaze's results would be about the same as that of the present study, the reliability coefficient of the results from his 16 *Ss* would be in the neighborhood of 0.74. Correcting the obtained correlation between the two sets of indices (0.48) for attenuation on this basis, we obtain an estimate of the true correlation to be 0.63. The value should be 1.00 if both methods measured the same process.

The difference between 0.63 and 1.00 could be attributed entirely to differences in technique only on condition that it were known that no other source of difference exists. As a matter of fact, it is very likely that the difference in geographical location from which the *Ss* were recruited might have an influence. There is also reason to believe that even a few years in time might make an appreciable difference, particularly in the catch words in general use. For example, the present results were taken just after the use of beer was legalized, and one of the syllables frequently called up the

name of a popular brand of beer. On the other hand, it seems likely that a substantial portion of the apparent difference may be due to differences in technique. The matter of spelling the syllables by *E*, employed by Glaze, as contrasted with the more usual method of having *S* pronounce the syllables, employed in the present study, is noteworthy in this connection.

In order to secure a little indirect light on this latter question, speech records were made of a naïve *S*'s spelling and pronouncing a selected list of 13 syllables from those here studied.[3] It was found that the spelling on the average required nearly 3 times as long as pronunciation. Moreover, with 11 of the 13 spelling reactions there was at least one interruption of vocalization, and in 7 cases there were 2 interruptions. This indicates that spelled reactions at the beginning of learning are generally chains of 2 or 3 objectively distinct reactions, whereas the typical nonsense syllable composed of a vowel between two consonants ordinarily presents a single motor unit. These facts, together with those presented above, would seem to indicate the desirability of having separate determinations of meaningfulness and memorization difficulty for each major difference in the experimental technique employed.

[3] The writer is indebted to Harold C. Taylor for this determination.

APPARATUS

THE GLARELESS LIGHTING OF A DARK ROOM

By C. E. Ferree and G. Rand, Johns Hopkins University

Among the conditions needed in lighting a dark room the following may be noted: (1) a total absence of light for dark adaptation and other work that requires the complete exclusion of light; (2) a low general illumination giving just the degree of visibility that is needed for the convenient use of certain instruments employed for the examination of the eye, for other work in which complete exclusion of light is not required, and in general for finding one's way about without the use of more than a minimum amount of light; (3) a moderately high illumination with a downward component for use when arranging and setting up apparatus, when cleaning the room, and for other work of various kinds; and (4) the complete absence of glare. This latter has ever been the difficult feature to obtain. To provide without glare a sufficient amount of light for all needs which arise, in a room whose walls, ceilings, and floor are especially surfaced for the purpose of the complete exclusion of light, has been one of the outstanding problems in special lighting. In this connection, it will be remembered that the sharp contrast with the black surroundings gives an intolerable glare to surfaces which might otherwise not be considered of objectionable brightness.

The problem under discussion came up rather acutely in the lighting of the four dark rooms in our laboratory. These rooms required to be lighted in a way that would be suitable for any type of clinical examination of the eye and for all kinds of experimental work requiring low illumination or the complete absence of light. For purpose of description one room will be chosen as an example. This room is 19.5 ft. long, 15 ft. wide, and 12 ft. high. The walls, floor, and ceiling are surfaced with an especially prepared, flat black paint. The dark rooms are all inside rooms and the doors are carefully light-proofed. To provide for ventilation each door is fitted with light-proofed louvers. Each room is also provided with a duct connected with a fan-ventilating system which supplies air from the outside and completely changes the air in the room in a very short time. The arrangement is such that the air from this system can be turned on and off in each room separately. Also especial care has been taken to sound-proof the ventilating system.

The lighting system is a combination of direct and indirect lighting graduated to give the correct proportion of distribution of light and brightness in the upper and lower halves of the room. In the center of the room is a fixture of the totally indirect type supplied with a 60-watt lamp and at each corner of a circumscribed rectangle, 6.5 x 9 ft., is a unit of the direct type (X-ray reflector no. 575) also supplied with a 60-watt lamp. The direct units are installed on the ceiling. The installation of the indirect unit is such that the top of the reflector is 30 in. from the ceiling. The two types of unit are placed on separate

* From the Research Laboratory of Physiological Optics, Wilmer Institute, Johns Hopkins Medical School.

circuits. The indirect unit is intended to give a low general illumination and the direct units the higher light that is needed at times on objects in the plane of work. Each of the direct units is provided with a diffusing plate and a glare baffle across the bottom. The diffusing plate which is positioned immediately above the glare baffle is of imported Belgian flashed opal glass. This glass has a high coefficient both of diffusion and transmission. It is smooth on both surfaces which adds to the ease of cleaning. The attachment of the diffusing plate and glare baffle is such that they can be easily removed for replacement of the lamp and for cleaning. As a further protection against glare we added to the indirect unit a downwardly extending canopy or glare shield which encloses all the bright parts of the socket and the neck of the lamp. This canopy, the means of suspension of the indirect unit, the outer surfaces of both types of unit, and all auxiliary parts and fittings were painted a flat black.

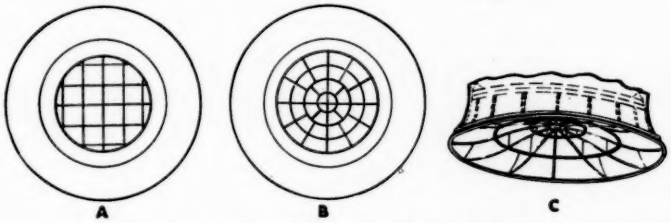

FIG. 1. SHOWING THREE TYPES OF GLARE BAFFLE

Three types of glare baffle have been used. One of these is shown in Fig. 1A. It has a cellular construction similar in appearance to an egg-crate. In fact egg-crates were used at first as baffles and are still in use in the room which is being described. The baffle consists substantially of two series of vertical partitions extending across the opening of the enclosure at right angles to each other. The partitions extending in each direction are parallel. They are in the present model spread apart by about 2 in. and are approximately 2 in. wide. Through the downwardly extending passages formed by these partitions the greater part of the light is allowed to pass to the plane of work, only such rays being intercepted as will produce glare in the eyes of those working or located in various probable positions in the room. To one directly beneath the unit, or nearly so, glare is experienced only when the eyes are directed upwards. In all other positions in the room the eye is protected in all directions. In order that the protection shall be complete, both surfaces of the partitions are painted flat black. When the unit is installed in the proper position in the room and at the correct distance from the ceiling, the opening as seen from the various positions in the room is either not luminous at all or has a dull luster or a soft, silvery sheen of such low brightness as not to cause annoyance or discomfort to the most sensitive eye.

In Fig. 1B the baffles are arranged in the form of concentric circles. Partitions circularly disposed, however, shield the eye completely only when the gaze is directed along the common radius of the circles. In all other directions glare is experienced. In order to shield the eye in all directions radial segments are inserted between the circles at suitable places. While good results are obtained with

this form of baffle, we have not found it to be so entirely satisfactory as the egg-crate type shown at A.

In Fig. 1C the partitions forming the baffle are both circular and radial. The radial partitions are curved so that the appearance is that of a bottom slightly convexed upward. Intersecting the radial partitions at suitable vertical levels are circular partitions. Either illuminated or dark, this baffle gives a very pleasing effect. Both it and the type of baffle shown at B, however, obstruct the downward passage of the light more than the cellular or egg-crate construction shown at A.

Besides serving a useful laboratory purpose, this experiment in type of installation teaches a very important lesson in the principles of lighting. It demonstrates more clearly than we have ever seen before, the incorrectness of principle in both direct and indirect lighting. A dark room with its dark walls, ceiling and floor which absorb almost entirely the lesser amounts of light throws into sharp contrast the parts of the room which receive excessive amounts of light and cause the distribution of brightness in the field of view to be out of balance. That is, when the indirect light is turned on alone the ceiling and upper part of the room are light and the lower part of the room is dark; and when the direct light is turned on alone the lower part of the room is light and the ceiling and upper part of the room are pitch black. When both types of unit are turned on together, however, we have the impression of entire comfort and restfulness and of correctly balanced light and brightness in the field of view. Arranged so that the three sets of conditions can be produced in quick succession, this installation gives a striking experimental analysis of what is wrong in direct and indirect lighting, and shows what is needed to remedy the defect; namely a combination of both principles in the same unit with a completely adequate glare baffle to eliminate annoying and harmful brightness in that member of the unit which provides the downward component of light.

A brief review and statement of principles may not be out of place at this point. Thus far ceiling fixtures have been of two types: (a) open bowls, opaque or translucent, installed pendant or inverted; and (b) closed bowls, or other enclosing housings of translucent material.

With an opaque reflector turned up (totally indirect lighting) the upper part of the room illuminated is too bright, especially when ceiling fixtures are used and high intensities of light are wanted on the working plane. With an opaque reflector turned down (direct lighting) the upper part of the room is too dark and the lower part disproportionately bright, and the glare from the opening of the reflector is fatiguing and harmful to the eye. With a translucent reflector turned up (semi-indirect lighting) harmful glare results from the unit itself when the reflector is of too low density and from the ceiling when it is of high density. With a translucent reflector turned down (direct light with a translucent shade) either the opening or the sides of the reflector or both are of harmful brilliance, the ratio of brilliance of the side and the opening depending upon the density of the reflector. With both the opaque and the translucent reflector turned down there is usually also a harmful glare from the work because of the directed beam of light. If an enclosing globe or housing of diffusing glass is used, a better distribution of light and surface brightness on walls and ceiling is obtained, but the glare from the source is cut down only in proportion to the absorption of light by the enclosing glass. Because of this limitation such units invariably have much too high a surface brilliance.

In lighting interiors three practices have been followed, singly or in varying degrees of combination, for reducing glare from the source: reflection, absorption and an increase of the area of surface acting as the source. Clearly the first two are effective only in proportion as they are wasteful of light. The third has reached its highest development in flood lighting and in the lighting of interiors with indirect lighting from floor and table stands, in which case the ceiling or some other large surface acts as a secondary source for illuminating the plane of work. It has never been made satisfactorily effective, however, with the use of any type of ceiling fixture.

It is obvious that in the lighting of interiors with ceiling fixtures, some type of glare baffle is needed that will protect the eye from glare without harmful waste of light. With the demand for higher intensities of light this need has become imperative. The use of higher intensities of light has also caused an increase of glare from the work. With a given character of working surface this type of glare reaches its maximum with a directed beam of light and its minimum with completely diffused light. In order to protect the eye from glare from the work, every lighting unit should therefore be provided with a diffusing member or some means of diffusing the light.

To give satisfactory results a ceiling fixture should combine the following features. (1) It should provide a means for illuminating the ceiling and the plane of work with light in the ratio of intensities needed to give a comfortable distribution of brightness in the field of view. This can be obtained by a suitable combination of the features of direct and indirect lighting. (2) Means should be provided for shielding the eye from glare from that part of the fixture which directs the light to the working plane. (3) The beam directed to the working plane should be diffused.

All the foregoing features have been combined by us in a unit, the idea for which came from observing the effects produced by the combination of units described above for lighting a dark room. The direct incentive for devising the unit was a request from the Director of one of our leading hospitals for a glareless unit for the lighting of hospital wards. The unit is not only well fitted for the lighting of hospital wards and is giving excellent satisfaction for that purpose but is also admirably adapted to the lighting of dark rooms. For that reason we think that it should be noted here.

Drawings of the unit are shown in Fig. 2A and B. The construction will not be described in detail. In A of this figure it may be seen that the upper part of the reflecting member is curved upward and outward to direct a portion of the light to the ceiling. The lower part of the reflector directs a part of the light from the lower hemisphere of the lamp to the opening in the bottom of the unit. The remainder of the light from the lower hemisphere of the lamp passes directly from the lamp to this opening. The direct and reflected light is received by a plate of glassware which extends across the unit at a suitable distance beneath the lamp. This plate which has a high coefficient of both transmission and diffusion serves to diffuse the light with a minimum loss by absorption. In the opening in the bottom of the unit, directly beneath the diffusing plate, is inserted the glare baffle. The type of baffle used here is that shown in Fig. 1A.

In order further to protect the eye from glare a downward canopy or glare shield is provided which encloses all the bright parts of the socket and the neck of the lamp, as was the case in the indirect unit described earlier in the paper. In

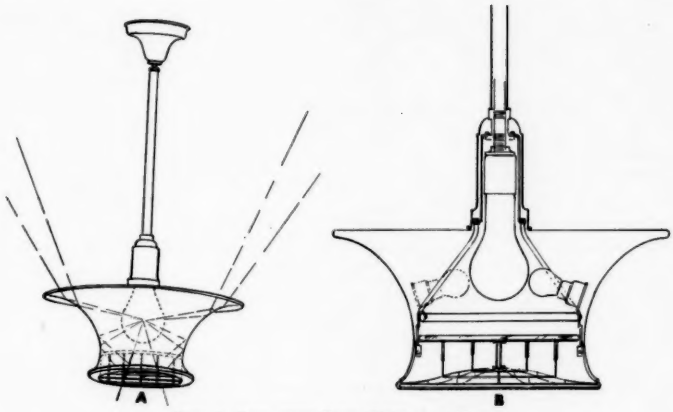

FIG. 2. SHOWING TWO TYPES OF UNIT
A shows unit without night-light attachment and with the glare baffle shown at A, Fig. 1. B shows unit with night-light attachment and with the glare baffle shown at C, Fig. 1.

the greater number of even the better lighting units in use at the present time, these have a high brightness and are objectionable and disturbing to the eye. This canopy which is attached to the lamp socket is shown in the illustration given in Fig. 2B. It may be painted in the same color as the outer surface of the reflector, in the present unit light ivory, so that it will harmonize with the reflector when the light is not turned on; or it may be painted a darker shade to harmonize with the reflector when the light is turned on. In the latter case a much better protection is afforded the eye. In units as uniformly glareless as these it is surprising to note how small a difference in brightness is noticeable and even distractingly annoying to a sensitive eye. For use in a dark room this canopy, the outer surface of the reflector, the means of suspension and all auxiliary parts and fittings should be painted a flat black.

The unit has also a provision for a low illumination, which in case of the hospital unit, is called a night-light attachment. The attachment supplies enough light for the inspection of the patient and the objects in the room. The light is in part reflected to the ceiling and from there diffused to the working plane, and in part passes through the diffusing member and glare baffles across the bottom of the fixture, directly to the working plane. When the light is turned on, no surface or object in the room has a brightness high enough to be sensed through the closed lids. The room is softly and uniformly illuminated with well diffused light of an intensity that is not high enough to waken a sleeping patient. The two sources of light, general and twilight, are on separate circuits and both can be operated from the corridor by an attendant or by the nurse in charge. This night-light attachment is of value in the use of the unit in the dark room to give the faint light that is needed for the types of work that require low illumination and in general to give enough light to see without greatly disturbing the state of adaptation of the eye.

The attachment is shown in Fig. 2B. It is mounted inside the upwardly extending reflecting member, a short distance above the diffusing plate. It consists of a circular brass tube on which at diametrically opposite points are mounted two small frosted lamps (2-candle power, 120-volt, Form I, S-8, candelabra base). The filaments of these lamps are at approximately the same vertical level as the filament of the lamp that provides the general illumination. The feed wires for the lamp are threaded through the circular tube. The tube is held in position by suitable mounts depending from the main socket of the fixture. In this figure is shown the type of glare baffle represented in Fig. 1C.

Two lamps were used for the following reasons. If only one were used, the lamp employed for general illumination with its auxiliary fittings, which remain in position when the night light is turned on, would cast troublesome shadows. Two lamps in the location described give a shadowless, well distributed illumination of optimum or very nearly optimum intensity. More can be used, however, if for any reason more light is wanted.

While the unit in question was devised for a hospital unit, the same features of glareless, well balanced distribution of light and brightness in the field of view give it merit for other lighting situations. Without the night-light attachment the unit serves excellently for commercial and home lighting. In combination with the night-light attachment it has additional uses in the lighting of bedrooms and other rooms and of halls and corridors in which a low as well as a high illumination may be needed.

The unit combines the features of diffused and glareless direct lighting with indirect lighting. The former provides for a strong downward component of diffused light without glare on desks, tables, instruments and other objects in the lower half of the room, and the latter gives a pleasing illumination to the upper half of the room and in addition an overlay of highly diffused and well distributed reflected light on objects in the lower part of the room. The purpose has been to secure in one unit the best features of both direct and indirect lighting in supplementary relationship. Neither type of lighting is complete in itself.

A means of illumination of this type for the dark room not only removes many of the annoyances and inconveniences ordinarily associated with dark room work, but it opens up new possibilities for the utilization of a standardized low illumination for many types of laboratory and testing work, for the use of certain instruments in the study of pathologic cases and for other purposes for which a low illumination is required.

A DEMONSTRATIONAL APPARATUS FOR THE PATELLAR TENDON REFLEX

By Robert G. Ruhl and Harrison Musgrave
University of Southern California

The following simplified apparatus for demonstrating the patellar-reflex measures the strength of stimulus, the extent of motor response, and the latent time in 1/120-sec.

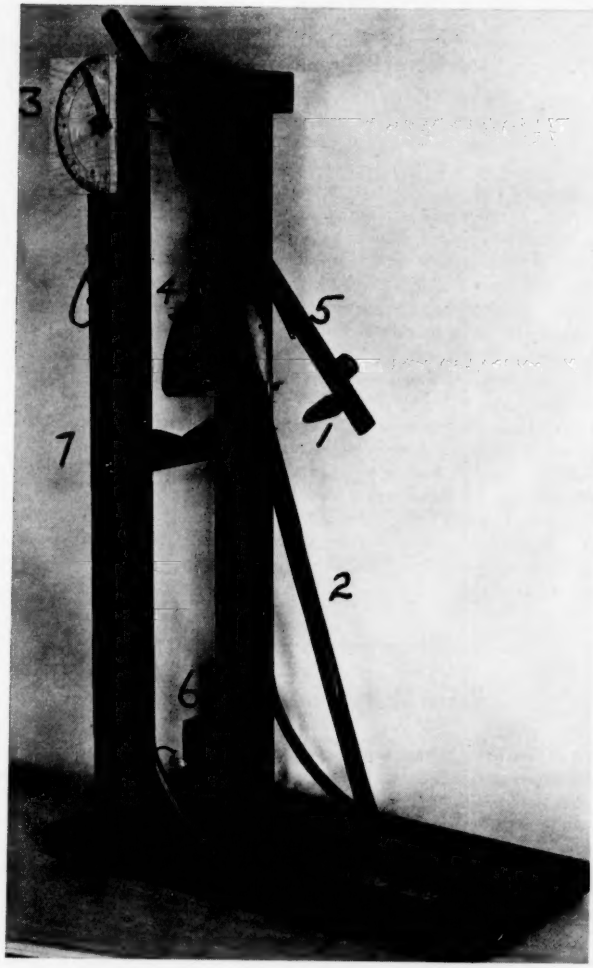

FIG. 1. A DEMONSTRATIONAL APPARATUS FOR THE PATELLAR TENDON REFLEX

The frame of the apparatus is made of 2 in. x 4 in. lumber braced with metal right-angle brackets. The bar (7) supporting the *S*'s leg (see Fig. 1) is hollowed on its upper side to hold the leg in a fixed position. A metal shaft inside the bar extends through vertical slots and is fitted with a wing-nut at either end to adjust the height of the leg. The hammer-head (1) is a wedge-shaped piece with a striking edge 3 inches long. The hammer-arm is slotted at the upper end to vary its length. It is fixed to a block with a wing-nut. This block is mounted solidly to a shaft which extends through, and which is free to turn in, the frame. One end of this shaft is fitted with a pointer (3) which moves over a scale. Thus a measure of the stimulus is indicated by the number of degrees through which the hammer drops. The extent of the leg-movement is measured by a lever (2) fixed below to the foot. The pivot around which this lever turns, with its angular scale, is adjustable up and down so that it may be centered opposite the knee-joint. The pointer (4) carried by the lever over its scale remains at the lowest point when the lever moves down and back. The pointer must be reset to zero for each trial. A wooden shoe, fastened with rubber bands to *S*'s foot, has a projection on one side which serves to move the lever.

The latent time is measured with a Klopsteg impulse counter.[1] The simplest arrangement is to mount a make-switch on the stimulus hammer-arm (5) and a break-switch behind *S*'s heel (6). Then the circuit through the counter is closed from the time of the stimulus-impact to the beginning of the leg-movement. A projection from the back of the wooden shoe holds this break-switch closed until movement begins. A more satisfactory method is to use two break-switches. The two-break circuit may be connected to the psychodometer, or to a relay similar to the one used with that instrument.[2]

A CONDENSER-CHARGE TIMER

By Wm. D. Moeder and John G. Jenkins, Cornell University

In an earlier note, reference was made to a condenser-charge timer which was being used to control tachistoscopic exposures in the Cornell Laboratories.[3] Inquiries from other universities have prompted us to present at this time the details of this device without waiting for more extensive checks on its accuracy. Meanwhile it may be noted that extensive determinations here and in two other psychological laboratories seem to indicate that its average error is of the order of one or two milliseconds (ms.) in 500. The timer is accurate, easy of construction, and inexpensive—a trio of virtues which may appeal to psychologists in general at the moment.

As employed by the authors, the two models in use at Cornell are laid out on small panels which are situated within easy reach of the experimenters. Reference to the schematic wiring diagram (Fig. 1) will reveal the necessary parts, which, it will

[1] P. E. Klopsteg, An impluse counter, tallying device, and electric stop watch: A useful laboratory instrument, *J. Opt. Soc. Amer. & Rev. Scient. Instr.*, 19, 1929, 345-349.

[2] M. Metfessel and H. Musgrave, *Instructor's Guide for Demonstrations of Psychological Experiments*, 1933, 3-4.

[3] J. G. Jenkins, A simple tachistoscope of many uses, *supra*, 150.

be obvious, are few in number and inexpensive. The 0.1 mfd. condenser is critical in value and should be a mica condenser of the best grade obtainable. The relay we employ is of the ordinary telephone make-and-make type. This operates well on the output of a 171-A tube with 90 volts on the plate, and its contacts are capable of carrying the 110 volt current used for both sets of lamps in our tachistoscopes. The blocking resistor (R_2) is employed only as a protective device and may be omitted without affecting the operation of the circuit. The key is easily constructed by adding an off-normal contact to any available key, thus permitting the discharge of the condenser after each exposure. Since filament voltages are critical, a voltmeter should be inserted in the filament lead. Needless to say, old batteries with high internal resistance cannot be expected to produce accurate results.

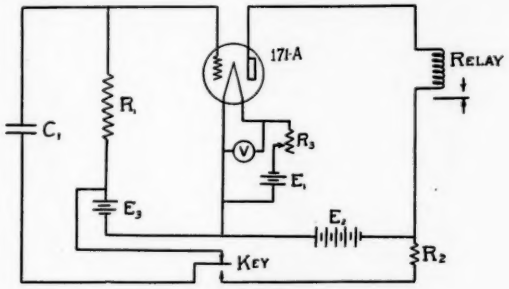

FIG. 1. WIRING DIAGRAM OF CIRCUIT OF CONDENSER-CHARGE TIMER

C_1 = 0.1 mfd. condenser
R_1 = critical resistor (½ to 20 meg.)
R_2 = blocking resistor (½ meg.)
E_1 = A-battery (6 volts)
E_2 = B-battery (90 volts)
E_3 = C-battery (16 volts)
V = voltmeter

Variations in time are obtained by employing as R_1 resistors of various values. Since the times obtained are a function of the characteristics of the individual tube employed as well as the resistors, we shall not attempt to give a table of time-equivalents. Although the device must be standardized for a particular tube, it may be of some value to know that the tube we are using at present yields almost a straight-line function from ca. 60 ms. for a 1 meg. resistor to ca. 300 ms. for a 10 meg. resistor.

The principle of operation may be briefly described as follows. Before the key is closed, the grid and negative C-battery terminal (E_3) are at the same potential; this is sufficiently negative so that the plate current will not actuate the relay. The experimenter now closes the key and holds it closed until the click of the relay informs him that the exposure is complete. While the key is closed, a charging current flows to the condenser through the resistor (R_1) causing the grid to become instantaneously positive with respect to its steady condition, and causing a plate current large enough to actuate the relay.

If the grid is brought to a positive potential at which a grid current will flow, the grid impedance will act as a shunt resistor to the resistance (R_1) and will hasten the charging rate of the condenser until the contact potential of the tube is reached. At this point the grid resistance may be assumed to become infinite, the only resistance affecting the charging rate of the condenser from this point to the point at which the relay will open being the resistor (R_1). Since our tube is operated at such values of E_1 and E_2 that the relay operates at a grid voltage less than the contact potential of the tube, and since R_2 is so chosen that the grid never actually becomes positive, the entire cycle tends to occur with the resistor (R_1) as the only controlling element. This reduces the chances of inaccuracies due to changes in tube characteristics. Since the charging current through R_1 decreases logarithmically with time, the grid potential becomes increasingly negative and a point is reached at which the plate current is insufficient to hold the relay closed, and it returns to its original position.

As stated before, the release of the key permits the discharge of the condenser by way of the off-normal contact, and the device is ready for the next exposure. The length of the exposure is in no sense a function of the time for which the key is held down, except that it must be kept depressed until the relay has released. A feature of the apparatus is its flexibility and wide range of possible application. It may be used to make and break a single circuit, or it may be employed, as it is here, to secure reciprocal make-and-break actions in two circuits. By changing the values of condenser and resistance appropriately, almost any range of times may be secured, so that the timer may actually be used as a delay-circuit for times even several minutes in length. At the very least, it can replace various laborious methods of securing accurate exposure times of extremely short duration.

NOTES AND DISCUSSIONS

Revision of a Biography

In 1923 I published a biography of Sigmund Freud,[1] which appeared in English translation in 1924, and which, because of the nature of the book, received wide attention in scientific circles. It is to the present day cited by psychologists in their lectures and publications, and I have reason to fear that a number of psychologists, rather remote from the field of psychoanalysis, derive a major part of their knowledge of this subject from my book. This has been a source of constant embarrassment to me for I have, in the ten years that have elapsed since the appearance of the book, changed my opinion considerably concerning psychoanalysis and its founder, and therefore can no longer stand sponsor for the errors and misrepresentations which I have come to recognize as such.

I have been repeatedly urged by my American friends to publish a revised edition of the book, and this seemed at first to be the simplest way out. As I went over the book, however, for this purpose, I realized that it would be a sheer impossibility for me so to transform the work that it would express adequately my present position in regard to Freud and his school, to which I belong. The tone of the book throughout is a striking example of ambivalence and it is beyond my power to entirely eradicate it. Passages in which I pay high tribute to Freud alternate with others which hurt—not only his work, a brunt which every scholar must bear, but his personality, passages which, I was later to discover, partly through my own observations, were based on false assumptions. The book is so written that it is perforce welcome to all who seek rationalization for their own ambivalent attitude toward psychoanalysis.

I have since given expression in various books and articles to my change of view, yet none of these works have been able to obliterate the impression that I created upon scientific circles by my publication of 1923. I had hoped and with some ground for the hope that the publication would gradually fall into oblivion. For this reason I kept silent, thinking it best to return no more to what I regarded as a sort of youthful indiscretion. While, however, the purely scientific portion of my book, with its truth and errors, scarcely needs further comment, it is precisely the personal content of the book which to my chagrin continually reappears.

I say in my book that Freud has an overweeningly good opinion of himself, the Jehovah complex; that he is a despot who will not tolerate deviations from his system; that he brings his disciples into hypnotic dependence upon him; that he repels his friends and especially if they are men of importance themselves; that he rarely abandons errors which are obviously such, is not free from cryptomnesia, and in his psychoanalytic policy pursues ways not always above reproach. These words of disapproval—condensed here to a few lines—are spread throughout my book, over 287 pages, and are again and again interspersed with frank hero worship. However, this does not serve to mitigate them; quite to the contrary. The reader inevitably arrives at the conclusion: Behold one who so honors him, and yet . . .

[1] F. Wittels, *Sigmund Freud: His Personality, His teachings and His School,* 1924, 1-287.

The letter which Freud wrote me after the German edition had appeared, and with which in an authorized abstract I prefaced the English translation, shows that he, astonishingly unmoved, scarcely noticed the defamatory passages and approached this biography, of which before its appearance, he knew nothing, with apparently complete objectivity, as though it had nothing to do with his person but only with a matter which was, in general, science, in particular, psychoanalysis. Yet he did more than that. I had been pursuing my way apart from his pupils for a number of years—five years of war had lengthened the time. After the appearance of my book, which certainly was anything but a good introduction, he drew me back to his school and gave me the opportunity to relearn and to see for myself whether my opinion of his personality and his teaching was correct or not. I may say that never have I met a worker who so completely and effortlessly disappeared behind his work.

(1) Jehovah. Before the appearance of my book there could be found, with the exception of scattered remarks in his works *The Interpretation of Dreams,* and *The Psychopathology of Everyday Life,* only Freud's *On the History of the Psychoanalytic Movement,*[2] which contained personal statements concerning the author. Later there appeared his autobiography which gave in terse, reserved manner, certain facts of his life.[3] It reads like a protocol that leaves it to the reader to decide for himself whether it is a great man who speaks or not, and how great he perchance may be. Among all psychoanalysts, myself included, he is perhaps the only one who is not intoxicated with the method and its results. It has always remained questionable to him whether psychoanalysis—a matter of which we, his pupils, are all firmly convinced—would change the spiritual face of the world. He has always considered himself a "foreign body in German intellectual life," has allowed himself to be convinced of the opposite by Thomas Mann only out of courtesy, and has proved himself right, as the most recent events in Germany show—temporarily right, we hope.

If one speaks of his world renown, he answers not only with the wisdom of Solomon that this is vanity, but he reminds us that many an "immortal" together with his theories, has been forgotten in an incredibly short time. Therefore, albeit I then wrote that he looked upon himself as Jehovah, it now seems to me that it is his disciples who bestow upon him a rank against which he does not defend himself because it is a matter of indifference to him. I had moreover at that time no other foundation for my assertion than some mistakes (Freudian "parapraxis") and interpretation of dreams which Freud had imparted from didactic motives.[4]

(2) The Analysis of Freud. I employ in my book psychoanalytic methods with which to interpret certain of Freud's actions. It is, however, an unwarrantable act to strip bare the living or even departed contemporaries whose relatives are still alive, by unveiling their unconscious without permission to do so. I did not, as Freud himself stated to me in his letter, carry this indiscretion very far. Yet, as a matter of principle, it is not right to do this, just as it is not right to operate upon a person against his will. I set here a bad precedent,[5] which has led others to follow

[2] S. Freud, *Collected Papers,* 1, 1924, 1-359.
[3] Freud, *Selbstdarstellung,* 1930, 1-25; *Gesammelte Schriften,* 10, 1925, 1-447; and *The Problem of Lay Analysis,* 1927, 1-316.
[4] See pp. 16 and 28 of my book.
[5] *Op. cit.,* 36, lines 17–19; 72, lines 18–26; 100 f.; 103 ff.; and 233, lines 2–20.

me in a rather malicious manner. Herein, there lies a temptation which—aside from ethical objections—readily leads to ways of error, for without the free ideas and associations of the victim the psychoanalytic truth cannot be reliably discovered. If, moreover, a polemic purpose enters into the undertaking, precious little of scientific glory is left.

(3) Orthodoxy. Except for a few ill-fated attempts, no university has yet dedicated a chair to the newly created science called psychoanalysis by its creator. Hence, any charlatan can with impunity call himself a psychoanalyst. It was the good fortune of Freud's theory that its founder possessed the power of organization to assure his disciples places of work. His power of organization maintained sufficient strength to surround the civilized globe when scientific interest in psychoanalysis awoke in all cultural lands. Freud's theory must be preserved from disintegration and dilution. The International Psychoanalytic Association attends to that with its numerous branches, its publishing house and periodicals. It must be left to Freud and his disciples, I do indeed believe, to decide what psychoanalysis is and how it can be learned. Freud said even as far back as 1914, of his former pupils: The earth has room for all, if only they did not disturb him and his work. These former disciples, who have changed their scientific points of view widely enough to separate from the psychoanalytic path of research, like Jung, Adler, Rank, themselves no longer maintain that they are psychoanalysts. Why, therefore, differentiate Freud's psychoanalysis as "orthodox," as if it were not a matter of scientific conflict and defense, but one of faith and bigotry? Here, I have, I am afraid, indulged in a thoughtless mockery of patient scientific labor carried on for almost forty years under great sacrifices—even monetary sacrifices—in the face of a hostile environment. All who do not recognize this work as scientific have a right to say so. It should, however, be at least understood that Freud's school itself continues to believe that it is engaged in a scientific pursuit and that it moves as honorably and freely in the service of the truth as any other school. On May 10, of this year, Freud's works, among others, were openly burned in Germany. In this action one sees plainly on which side orthodoxy lies. Many others would consign Freud's works to the bonfire were they not ashamed to do so. The particular sort of resistance to the teachings of psychoanalysis, which "disturb the world's slumber," has been so often set forth by us that I need not revert to it here.

I state in my book that Freud holds his consultations behind closed doors. Naturally, he does so. What purpose could it serve after so many years of specialized work to admit people who are the more vociferous the less they understand? But when, because of this, and because he, allegedly, as I state in my book, "tolerates no deviation from his theory," I called him a despot, I wronged him. On reading our periodicals one may see how violent and active is the scientific dispute within the borders of psychoanalysis. Every doctrine adheres to certain fundamental principles, otherwise it would not be a doctrine. We psychoanalysts have united upon certain basic facts, like repression, resistance, transference, the Oedipus complex and its derivatives, the repetition compulsion and others, and we feel that we waste our time when we strive with those who have inadequate knowledge of our theories over the validity of discoveries long tried and tested. Every petty chief of a medical or psychological institute that is conducted according to his ideas is more of a despot than is Freud. I can only hold myself up again as an example of this, when

Freud, after the appearance of my, to put it mildly, imprudent book, drew me back to his fold.

(4) Cryptomnesia. I furthermore wish to retract my accusation of cryptomnesia.[6] Under the influence of ambivalence, I charged one of the most original thinkers of our time with plagiarism, albeit unconscious plagiarism. To be sure, we find ideas of Freud in Nietzsche and Plato, but such thoughts are in the very air that we breathe. No student is shut up in a vacuum; he founds his ideas always upon the concepts of others. Alfred Adler, in his own fashion, described the aggressive instinct and pursued the psychology of the ego before Freud had definitely comprehended and elaborated his own theories on the subject. Freud, however, grasped this problem so much more profoundly when he did recognize it, and was able, moreover, to subject it so completely to his own theory, that one can hardly say that he was influenced by Adler. Still less was he influenced by C. G. Jung when he wrought the "higher nature of man" into the magnificent conception of the superego.

(5) Psychoanalytic policy. I should like to add to what I have said in (3) above, an example of Freud's psychoanalytic policy as given in by book,[7] and present it in a new light. I refer to Freud's attempt in 1910 to put the leadership of the psychoanalytic movement into the hands of the Swiss. I quote from my book what he said at that time to his pupils in Vienna: "Most of you are Jews, and therefore you are incompetent to win friends for the new teaching. Jews must be content with the modest rôle of preparing the ground. It is absolutely essential that I should form ties in the world of general science. I am getting on in years, and am weary of being perpetually attacked. We are all in danger." Seizing his coat by the lapels, he said: "They won't even leave me a coat to my back. The Swiss will save us—will save me, and all of you as well." I no longer know precisely why I cited just these words of Freud's. Perhaps I desired to show that the great man under certain circumstances was faint-hearted. Today I am glad that I have rescued this episode from oblivion, for every one is aware at the present time, since the latest events in Germany, that twenty-three years ago, Freud had truly prophetic vision and that his fear was not for his own life but for the life of his teachings; he recognized a danger to its dissemination which at that time he alone realized. It is, however, one thing to recognize a danger and another to flee it. Freud could not escape it; he had to fulfill his destiny.

(6) Freud's disciples. That Freud's disciples, that is, the members of the International Psychoanalytic Association, are better and above all, different from the description of my book, may be apparent, perhaps, from the fact that today I count it an honor to number many of them among my friends. One must not, on reading my biography of Freud, fail to observe that the author at that time was unawaredly unfolding the psychology of Lucifer. The five years of war service had also made me somewhat bitter perhaps. Chiefly, however, I stood when I wrote this book under the influence of Wilhelm Stekel, who had analyzed me a short time before. I, together with most analysts with a sense of responsibility, who know the power of "transference" in analysis, would urge their analysands to desist from writing a book in which their analyst plays a chief rôle. Stekel did not do this. In some pas-

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[6] *Ibid.*, 195, line 28; 201, line 6.

[7] *Ibid.*, 139 f.

sages of the book can be found doubt of my objectivity, and expression of my own feeling that I was unduly influenced by Stekel. Thus on page 217: "I agree that I may be prejudiced in his favor, and that I am not in a position to give an unbiased opinion in this affair of Stekel versus Freud." Nevertheless, Stekel is glorified in my book. Today, I would no longer write in this vein. I am not in a position to say anything concerning Stekel. It is not meet to attack one's former friends, it justifiably creates a bad impression. However, I may say that I certainly no longer regard him as the "most successful and most distinguished disciple of Freud and his logical inheritor."[8]

(7) Suicide of analysts. I think that I should make further explanation of my statements concerning the suicide of analysts. I spoke of Otto Weininger, the author of the famous book entitled *Geschlecht und Charakter,* who shot himself. He died in 1903, hence before it was in any way possible for him to have been influenced by psychoanalysis as a therapeutic means, nor did I state that he had been.

Of the three other analysts whom I mentioned as having taken their own lives, not one of them had been analyzed, which was the chief reason for my having brought up the matter. An analyst who has not himself been analyzed is in danger, be it suicide or otherwise. One can refer to this in my book.[9] A disagreeable feeling was aroused among my colleagues by the following passage: "Three among the distinguished psychoanalysts I have personally known . . . ended their own days. . . . .Others may follow in their steps." The last sentence was interpreted as a more or less veiled challenge for some analysts to go hang themselves. I do not actually know whether in those bygone days, the will to a malicious challenge was not present rather on the side of my colleagues, but I may say with gratification that today such a sentiment is no longer attributed to me. Nevertheless, I expressly state that it was my desire to emphasize and over-emphasize how important it is for analysts to become aware of their own complexes before they call forth those of other people and permit these to react upon themselves.

In conclusion, I merely wish to state that psychoanalysis has since 1923, in which year my biography was written, made advances which I may compile in the near future and publish separately. It would not be fair to compress this important material into the frame of a short report which is chiefly designed to revise and rectify a previous publication.

I do not believe that I shall have recommended my book through this revision, but such, after all, is not my purpose. The book is too much an outburst of temperament to be scientifically above reproach.

New York City FRITZ WITTELS

THE DEPENDENCE UPON CHANCE FACTORS IN EQUATING GROUPS

In a recent analysis of experimental studies of learning and memory the writer was impressed with the frequency with which students in two sections of the same college course were used as control and experimental groups in psychological investigations.[1] The validity of the assumption that two groups of 25 or 30 students

[8] *Ibid.,* 17, 225. [9] *Ibid.,* 67 f.

[1] S. M. Corey, A summary of certain factors in current investigations on learning and memory, this JOURNAL, 44, 1932, 190-192.

each represent comparable average abilities has been questioned before. The following statistical manipulations were undertaken to present empirical evidence bearing on this point.

One thousand cards were marked with I.Q. in such a manner as to represent a normal distribution of intelligence. All of these cards, shuffled thoroughly, were then dealt four times, (1) into 40 groups of 25 cards each, (2) into 20 groups of 50 each, (3) into 13 groups of 75 each, and (4) into 10 groups of 100 cards each.

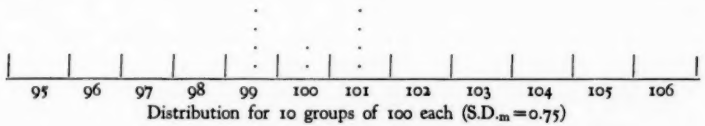
The purpose of these chance divisions of the large symmetrical group into smaller groups was to determine whether the various smaller groups of any given size would be comparable with respect to the mean I.Q. If they were, then it would seem reasonable to believe that other groups of the same size, selected by chance from a large symmetrical group of approximately 1000 would represent comparable average abilities. In other words, the use of two college classes as control and experimental groups, provided the classes were selected by chance from a much larger symmetrical group, might be justifiable.

The mean I.Q. for the 1000-card group was, of course, 100. The most probable expectation for the mean of each of the smaller groups would be the same. The degree in which this expectation was actually fulfilled is as follows:

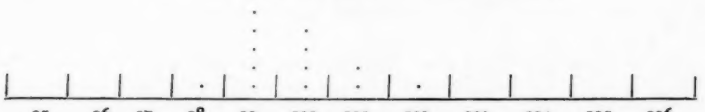
Size of group	Number of groups	Mean I.Q. for all such groups	S.D. of means for such groups
25	40	100	2.34
50	20	100	1.20
75	13	100	1.08
100	10	100	0.75

Here are shown the results of averaging the means for the 40 groups of 25, the 20 groups of 50, etc., as well as the standard deviations for each distribution of means. The departure from theoretical expectations was very slight, even in the case of the smallest groups. As the groups increase in size they become more comparable, *i.e.* the S.D. of the means decrease. The $S.D._m$ for the ten groups of 100 is less than one-third as large as that for the 40 groups of 25.

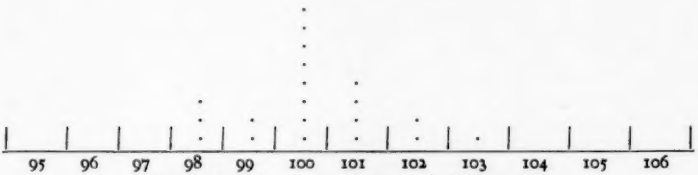
The following figure gives the distribution of the mean I.Q. for each of the size-groups.



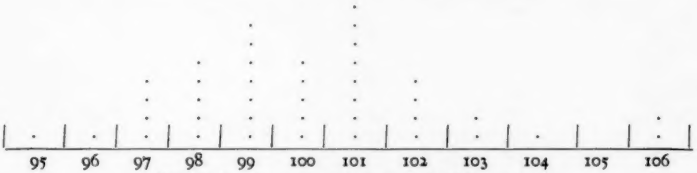
Distribution for 10 groups of 100 each ($S.D._m = 0.75$)



Distribution for 13 groups of 75 each ($S.D._m = 1.08$)



Distribution for 20 groups of 50 each (S.D.$_m$ = 1.20)



Distribution for 40 groups of 25 each (S.D.$_m$ = 2.34)

The difference between the two extreme means of the group containing 25 cards is 11 ± 3.3. These two extreme groups, although selected by chance, are obviously not comparable. But in any chance selection it might be very improbable that two such extremes would be paired for experimental purposes. In order to determine further whether groups of 25 selected under the circumstances of this study are comparable, the means for all such groups were placed on cards and put in a box. Two of these means were then selected at a time until 94 such selections had been made. The two means for each of the 94 pairs were then compared to see if significant differences existed between them. A difference was considered to be significant when it was three times its probable error. The result of these comparisons is given below in terms of the number of chances in 100 favoring the conclusion that a significant difference existed between the two means selected at any one time.

CHANCES IN 100 THAT THE DIFFERENCE BETWEEN THE MEANS OF ANY PAIR IS SIGNIFICANT

	50–54	55–59	60–64	65–69	70–74	75–79	80–84	85–89	90–94	95
No. of such pairings	9	12	14	9	7	13	6	7	8	9

Although none of the differences was three times its standard error, there was a large number of cases (43) wherein the odds were anywhere from 3 to 1 to 10 to 1 in favor of a significant difference between the means compared.

It would appear from the above demonstration that the practice of using various sections of the same course in psychology for experimental and control groups is unsound. Admitting the convenience of this procedure, the probabilities are that fairly significant differences might have existed between the groups with respect to the variables studied prior to any experimentation. This is particularly true when the classes used include no more than 25 individuals. The magnitude of these differences between actual sections would be greater than those apparent in any manipulation of a normal distribution of 1000 cards. This is true for a number of reasons. First, the sections of a University course-group are rarely recruited from a group as large

as 1000, and the smaller the original group the more likely will smaller groups selected from it show asymmetry. Secondly, even though attempts are sometimes made to bring it about, the allocation of students to the several sections is seldom a matter of chance. The instructor, the hour of the class-meeting, and the other persons to be in the class are all selective factors. Finally, no matter how large the original group may be, it will probably not represent a symmetrical distribution of abilities, a fact which would of course affect the symmetry of the smaller groups too.

University of Nebraska STEPHEN M. COREY

THE EIGHTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The Midwestern Psychological Association held its eighth annual meeting on Thursday, Friday, and Saturday, May 18, 19, and 20, at the Iowa State College, Ames, Iowa. All of the sessions took place in the Memorial Union Building except the public address of Professor C. Spearman and the Symposium on Industrial Psychology which were held in the Physics Auditorium. About 350 psychologists were registered for these meetings and they came from a widely distributed geographical area. Professor Horace B. English of Ohio State University officiated as president. Altogether there were six sectional meetings with two sessions running simultaneously throughout Friday and on Saturday morning during which 38 papers were read. These had been selected by the program committee from more than twice that number submitted. In addition during the usual Saturday afternoon session devoted to reports from laboratories, representatives from ten institutions gave a synoptic account of the research work under way.

The meetings opened with an address by Professor Harvey A. Carr (Chicago) on "The quest for constants" in which he summarized various attitudes customarily taken in learning experiments and made a comparative study of results obtained. He concluded with the concept of numerous specific learning curves in place of one stereotyped form of the curve. It is hazardous to assume that data obtained in connection with experiments on learning and forgetting could be treated independently of the specific conditions surrounding each experiment. Over 150 persons attended the lecture which was given in the Great Hall of the Memorial Union Building. On Friday evening the annual dinner was held with Dean Carl E. Seashore (Iowa) serving as toastmaster. President R. M. Hughes (Iowa State College) gave the address of welcome and Dean Charles E. Friley (Iowa State College) read a paper on "Psychology in a technical institution." The address of the President, Dr. Horace B. English (Ohio State), followed. He dealt extensively with the dualistic notion of the mind-body relationship and proposed a type of psychology in which the activities of the entire organism would hold a major place. At the close of the address the Association was pleasantly entertained by Mr. Charles Cooney, a professional magician, of Ames. This staged performance was camouflaged on the program under the title, "Practical demonstration of visual perception with attentional misdirection" and its real nature was known only to the committee. The annual business meeting was held on Saturday afternoon during which reports from the officers and from the Executive Committee were read and approved. The invitation of Purdue University to hold the next meeting of the Association at Lafayette, Indiana, was accepted. Two informal symposia were organized, one on

learning, under the chairmanship of Professor H. B. English (Ohio State), attended by 30 persons, and the other on emotions, under the chairmanship of Professor Christian A. Ruckmick (Iowa) with over 50 persons in attendance. In the symposium on learning the chairman raised the methodological question of proceeding from relatively gross forms of learning response and by experimental means breaking these down into component parts in contrast with the effort to begin with the supposed neurological basis of learning and working up to discover their function in the learning process. During the discussion it was of interest to note several concrete instances of retention in which there was apparently no evidence of forgetting taking place. The symposium on emotions centered on matters of definition of emotion and the possibility of distinguishing one form from another in terms of electrical manifestations from the body. The chairman pointed out that the more primitive forms genetically considered gave a more definite response. Discussion also revealed the possibility of using moving stimuli, such as the motion pictures, to produce effective emotions. The group was interested mostly in matters of technique and procedure and in the description of results thus far obtained.

About 250 persons listened to an elaborate symposium on industrial psychology which was held on Saturday evening under the chairmanship of Professor J. E. Evans (Iowa State). The following four papers were read: "The analytical method of job-grading," F. A. Kingsbury (Chicago); "Measuring the ability to operate automotive vehicles," A. R. Lauer (Iowa State); "Psychology and occupational adjustments," D. G. Paterson (Minnesota); and "The riding qualities of an automobile," A. Swope (Purdue). Two principal techniques of job-grading commonly employed in programs of standardization of salaries were compared by Dr. Kingsbury. The possibilities and limitations of the analytical technique were discussed in terms of a recent classification of some 500 office positions based on three aspects referring primarily to duties and responsibilities and on four items representing minimal essential qualifications of the position analyzed. Dr. Lauer presented the results obtained by the use of specialized spectrophotometric methods in testing drivers of automotive vehicles. Normal subjects over-estimate the short wave-lengths and under-estimate the long wave-lengths when the stimuli are of equal intensity. Color-blind persons match colors independently of intensity with almost the accuracy of normal individuals. The psychological ratings of 3700 individuals over a period of one and a quarter years were discussed by Dr. Paterson. He found that the general traits, including education, ability, and interests, averaged lower in the unemployed than in the employed classes and pointed out that psychologists will play an increasing rôle in employment agencies in the future. Professor Swope outlined five studies which he had completed on the mental and bodily effects of the riding qualities of automobiles. The mental effects come largely from the elimination of noise, increased comfort of sitting, and the increased field of visibility. He devised a "steadiness meter" to test individual differences in driving and found that adaptation to a definite type of car produced lower readings.

Preceding this symposium Dr. Spearman in a public address outlined his program of testing one thousand school children with a battery of 95 measures under the title, "The plan now in operation for an extensive investigation of unitary traits in ability." The actual testing in accordance with this program is actually under way now at the Mooseheart Laboratory. Dr. Spearman briefly explained his three principal factors of *g*, *o*, and *p* which are used to designate inherent common

modes of response. A special exhibit of lantern slides, motion pictures, apparatus, and demonstrational charts was arranged for in connection with the meetings.

By way of diversion the Psi Chi national fraternity held a luncheon in the tea room of the Home Economics Building for its members and guests on Friday noon. In the afternoon of that day the wives of the faculty of the Department of Psychology and friends served tea to members of the Association and guests in the Modernistic Room of the Memorial Union Building. A special trip to the Ledges State Park was also provided for on Saturday afternoon by the wives of the faculty in the Department of Psychology.

Section A under the chairmanship of H. A. Carr (Chicago) opened with a paper by H. Gulliksen (Mooseheart) in which he developed a new rational equation of the learning curve which synthesizes three differential equations that have previously expressed Thorndike's law of effect. It makes possible an analytical distinction between initial attainment and learning ability in a given task and also analyzes the effect of the amount of initial training upon the degree of transfer. N. Reiter (Wisconsin) followed with a paper on "The relation of gross bodily movement to problem-solving activity." She showed the combined results from different subjects who were seated in a chair while they were solving as rapidly as possible certain standardized tests involving arithmetical, algebraic, and logical problems. The movements which occurred while the correct answer was given and also those which occurred during the prescribed period when the correct answer was not forthcoming were traced. "Similarity in form of original and interpolated learning and retroactive inhibition" was the title of the next paper by Z. Peel (Arkansas). Paired associates and serial learning were used as techniques. Both experiments showed that retroactive inhibition was definitely greater when the original and the interpolated learning were of the same general form. H. N. Peters (Missouri) discussed seven series of experiments on mediate association. The conditions were controlled by instructional inhibition of the common item, by its prior meaningful connection, and by the Ebbinghaus technique of testing for remote association. In no case was positive evidence for mediate association shown. This was followed by a study on "The retention of interrupted maze performance" presented by F. McKinney (Missouri). With 50 *Ss* learning the mazes while interrupted during their performance and 50 learning them without interruption, his results show that there was no significant increase in retention due to the interrupted procedure. R. N. Menzies (Wisconsin) investigated the memory for pleasant, unpleasant, and indifferent events in recent past experiences. Changes in affective tone were correlated with the amount of forgetting. The last paper in this session established age and sex norms as bases for determining the relative performances of siblings in tests of mechanical aptitude. Extreme differences among individuals were discovered.

Running concurrently with Section A, Section B under the chairmanship of F. C. Dockeray (Ohio State) opened with a paper on pigmentation and visual sensitivity by J. P. Guilford (Nebraska). After determining the mimimal thresholds for light in 82 white subjects and 25 negroes he discovered that consistent differences were found both in white and negro subjects. The greater the pigmentation of the iris the greater, as a rule, was the sensitivity. L. Kardos (Mooseheart) followed with a study of the development of visual perception. The problem centered

around the "whole" character of 23 outline figures as over against the piecemeal character in the way that they were described or drawn. Eighty-six children of both sexes between the ages of 6 and 15 served as subjects. D. M. Purdy (Kansas) then presented a paper on "A case of chromatic agnosia and its modification with time." The subject when first examined could not match colors but could distinguish chromatic stimuli from achromatic stimuli. He lacked any concept of hue. Two years later he was able to match colors with considerable precision even though he still lacked the concept of hue. R. N. Gault (Northwestern) asked to have his paper on "Eliminating hearing in experiments on the tactual interpretation of speech and music" read by one of his students, L. D. Goodfellow. The effectiveness of sound-eliminating equipment was measured and was found to produce an effect equivalent to a hearing loss of 100 db. This was checked with 16 *O*s in 3200 trials. Later 14 naïve *O*s were entirely unable to identify five vowel qualities with this equipment. The extra-auditory sensations in the ear arising from stimulation by excessively great sound-pressures were discussed by S. N. Reger (Iowa). Tickling, pressure, and pain arising from sound pressures of about 1000 dynes per sq. cm. were experienced by 27 *O*s, who were completely deaf, at the same values that 46 normal *O*s did; but the threshold of these sensations was raised 25 db. or more in 33 ears lacking tympanic membranes. H. Woodrow (Illinois) presented data in connection with errors in weight discrimination. He made a comparison between two procedures, both variations of the method of right and wrong cases, but in the first fixed standards were used, while in the second the standards varied irregularly from trial to trial. The resulting time-order errors differed radically with the two procedures.

On Friday afternoon Section C, R. M. Elliott (Minnesota) presiding, opened with a paper by L. K. Henry (Iowa) on the rôle of insight in plane geometry. With a series of stimulus-cards, which gradually developed theorems and original propositions, no insightful behavior in geometrical problems was found in the 32 *O*s used. H. H. Remmers (Purdue) then discussed the validity, reliability, and halo-effect of human judgments in defined situations. A high degree of reliability obtained through 20 samplings of r for a population of 60 high school teachers and 60 college teachers and a high measure of validity, measured by the absence of the halo-effect, resulted. R. W. Husband (Wisconsin) obtained negative results in his study of the value of the photograph on the application blank when judged by large groups of college men and women and of business men. One hundred individuals were rated on 18 traits. The only value of the photograph which remains is its use as a check on race, facial disfiguration, and other matters of social importance. A technique for the measurement of popularity in pre-school children and some related factors was discussed by H. L. Koch (Chicago). By the method of paired comparisons 17 4-yr.-old nursery-children were tested for the rôle of the position-error, sex-preference, ennui, intelligence, and popularity. Various forms of tests were used in the analysis of these traits. H. B. Carlson (Chicago) used Thurstone's attitude-scales in correlating intelligence and student attitudes in 215 seniors. Women were found to be more favorable to prohibition and to have a stronger belief in the reality of God than men. Jewish students were the most liberal, Protestant students intermediate, and Catholic students least liberal. Intelligence was found to be positively correlated with communism, birth control, pacifism, and atheism. The last

paper in this session on the effect of practice on growth-curves was read by H. B. Reed (Kansas State) in the absence of the writer, E. B. Greene (Michigan). It was given only in abstract form. A number of test procedures were applied to 300 individuals ranging from 5 to 25 years of age. Some of the conclusions showed less improvement on the simple than on the complex tasks, higher correlations between subsequent trials with practice, and between adjacent trials on the easier tasks than on the harder ones.

Section D ran concurrently with Section C under the chairmanship of H. H. Remmers (Purdue). A. G. Bills (Chicago) read the first paper on "Stuttering and mental fatigue." On comparing the continuous perceptual-motor reactions of 25 stutterers with those of 25 normal subjects in the naming of colors, he found that stutterers tended to block more than twice as often and for longer periods than did normal individuals. Their reaction-times were irregular, they fatigued quickly, and their normal records closely resembled those of normal individuals when the latter were extremely fatigued. L. E. Travis (Iowa) followed with a study on the dissociation of homologous muscle function in stuttering. Action-currents were recorded simultaneously from the two masseter muscles during stuttering and during the free speech of both the stutterer and the normal speaker. The results indicated that the two sides of the speech-mechanism probably receive similar innervation during normal speech while they receive dissimilar innervation during stuttering. S. R. Hathaway (Minnesota) took parallel records of action-potentials from four muscles during simple voluntary movements and found that antagonistic muscles of a single pair contracted simultaneously. Single motor units often discharged periodically while the subject was relatively relaxed. The next paper was by L. E. Misbach (Northwestern) on "Modification of the knee-jerk by a concomitant voluntary reaction." His subjects were required to respond either with a right- or a left-hand finger movement in accordance with the brightness of a stimulating light. At the same time knee-jerks were produced during all stages of the voluntary reaction. Photographic records revealed the fact that the magnitude and form of the knee-jerk varied according to the stage of development reached by the voluntary reaction at the time that the knee-jerk occurred. A. Silverman and L. Baker (Wisconsin) next presented a paper which attempted to condition various responses to a subliminal electrical shock. A relationship was discovered between this stimulus and the increase in the amount of breathing, in the frequency of the eyelid response, in the muscular tension of the right leg, and in the amount of gross bodily movement, all of which responses were produced voluntarily by *O*. A. Jenness (Nebraska) closed this section with a discussion of the facilitation of sleeping hypnosis by previous motor response in the waking state. His results seemed to indicate that with easily hypnotized subjects a response to the suggestion of sleep facilitated the response to the suggestion of postural changes.

The first session on Saturday morning, Section E, under the chairmanship of R. H. Wheeler (Kansas), opened with a paper by G. Finch, E. A. Culler, and E. S. Girden (Illinois) on the "Relation of the Wever-Bray effect to auditory acuity in dogs." When the round window of the cochlea is closed with a soft plug, sensitivity to auditory stimuli is definitely decreased about 10 db., and the electrical pick-up from the homolateral nerve is reduced simultaneously by the same amount. W. C. Halstead and F. Fearing (Northwestern) next presented a paper on the incidence of central degeneration following labyrinthectomy with reference to the variability

of behavior-symptoms. The non-acoustic labyrinth was excised and a post mortem histological examination was performed. There was no evidence of central degeneration. The marked variability and temporal pattern in post-operative behavior can, therefore, not be explained by progressive degeneration of the central nervous system. By means of charts and slides H. F. Harlow (Wisconsin) described the capacity of monkeys to solve crossed-string problems. The monkeys showed levels of response equal to the human infant two or three years of age. Following the motion-picture film shown last year W. N. and L. A. Kellogg (Indiana) presented another film depicting the behavior of an ape and a child brought up under approximately the same conditions and of about the same age. The development of walking, affectionate behavior, socialized play, reactions to water and behavior in a baby-buggy were all shown and described in a very interesting fashion. D. R. Disher (Ohio State) then read the results of an experimental study of the reactions of new-born infants to olfactory stimuli. Thus far no racial differences have been discovered in sensitivity to a number of different smell stimuli. But there are large individual differences from one infant to another and from day to day in the same infant. The next paper on the differential responses of new-born infants to different degrees of intensity of light and sound was given by L. A. Weiss (Iowa). She studied 90 new-born infants under different intensities of light stimuli and 42 under different intensities of sound. Positive results were obtained for the most part regarding the effect of these stimuli on the amount of activity and on the movements throughout the stimulus period. The last paper in this section was contributed by W. L. Valentine and I. F. Wagner (Ohio State) concerning the relative motility of the arms of the new-born infant. Studying 55 infants over a period of one to ten days from birth they found no differences as related to color, sex, body-weight, head position, strength of grasp, or ancestral handedness. There is a slight preference for the right arm and the vertical component of the movement predominates in the right arm while the lateral component is most prominent in the left.

H. Woodrow (Illinois) presided over Section F which ran simultaneously. M. A. Tinker (Minnesota) read the first paper in this session on the "Use and limitations of eye-movement measures of reading." While he agreed that the photographic technique is the most satisfactory method of measuring eye-movements and that these measures are valuable, he is convinced that perception-time and the fixation-frequency are important in most quantitative comparisons and that ineffective habits in making eye-movements are symptomatic rather than causal in reading disability. The same subject was continued by R. Y. Walker (Iowa). He discussed the norms of seven measures of eye-movements with 50 students who ranked in the top decile of reading and other tests. Varying difficulties of reading material and requirements of comprehension gave significant differences in eye-movements for duration of fixation, size of fixation, and rate of reading. Mode of duration and extent of forward shift remained fairly constant. The next paper was read by G. A. Kelly (Kansas State) in connection with the relation of cerebral dominance to the perception of symbols. He came to the conclusion that this neural disintegration is a functional disorder of the nature of transient aphasia. This is further established by a study of difference in the orientations of the mnemonic images governed by the two cerebral hemispheres. D. A. Worcester and B. Silkett (Nebraska) followed with a study of the distance from eye to work among school children engaged at various tasks. After examining a great many children in kindergarten

through the sixth grade children while normally engaged in various tasks, the investigators conclude that it appears to be futile for teachers to attempt to establish a standard distance at which the children's eyes should be from their work. By well-standardized checking techniques K. Hevner (Minnesota) was able to establish differential mood effects for the major and minor modes respectively in music. With 205 listeners in connection with 10 compositions representing a variety of musical forms, each one of which was transformed from its own key to the other key, the descriptive terms which the *Os* used confirmed the usually assumed characteristics of the two modes. D. Lewis (Iowa) read the last paper describing the tone integrator, an apparatus for the study of timbre. This ingenious device, working through a photo-electric cell, makes it possible to synthesize a number of independent wave-forms in such a way that differences in timbre may easily be detected through a loud-speaker.

Under the chairmanship of C. A. Ruckmick (Iowa), who replaced S. Renshaw (Ohio State) in the latter's absence, a synoptic review of the laboratory work done at various institutions was obtained. Reports were made by the following persons: J. P. Guilford (Nebraska), C. E. Seashore (Iowa), A. G. Bills (Chicago), J. P. Porter (Ohio), R. Stagner (Wisconsin), H. H. Remmers (Purdue), H. B. Reed (Kansas State), D. P. Boder (Lewis Institute), H. Woodrow (Illinois), J. C. Peterson (Kansas State). The order of reports was determined by chance. Some interesting pieces of apparatus were explained, ingeniously improved techniques were described and a significant development of test-material was noted.

University of Iowa CHRISTIAN A. RUCKMICK

THE CURVE OF FORGETTING

Dr. S. K. Chou[1] states that Ladd and Woodworth were the first to express graphically the results obtained by Ebbinghaus in his fundamental experiments on retention and forgetting as a function of time. This is an error. It is true that the 'curve of forgetting' is not found in the work entitled *Über das Gedächtnis,* but it appears in the same author's *Grundzüge der Psychologie* on p. 646 of the first edition, 1902, where it serves to express the falling off of associations, in other words, forgetting. The term adopted by Ladd and Woodworth, by Thorndike, and by others after them, is consequently entirely justified.

J. LARGUIER DES BANCELS

University of Lausanne, Switzerland

A CORRECTION

A regrettable and curious error occurred in a book review in the July issue of this JOURNAL (p. 574). The manuscript of the review of Burnham's *The Wholesome Personality* is correct. Somehow a substitution of 'Wholesale' for 'Wholesome,' made in the galley proof, escaped detection by the editors. The compositor who set the type must have been so much set for 'sale' as to take this lead-off from 'whole' despite the objective stimulus before him; or shall we leave the interpretation to the psychoanalyst? The author can, we hope, find consolation in the fact that the person who made the substitution had not read the book.

JOSEPH PETERSON

[1] S. K. Chou, What is the curve of forgetting? this JOURNAL, 45, 1933, 349.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The House that Freud Built. By JOSEPH JASTROW. New York, Greenberg, 1932. Pp. x, 293.

Under a sustained figure of architectural construction, Dr. Jastrow describes the design and elaboration of the Freudian doctrines and practices; plans and specifications, building materials, superstructure, and uses. This fills the first part of the book. The second part is critical. It sets psychoanalysis against science; it analyzes the Freudian argument; it dissects the technique of practice; it sums up in judicial temper the entire doctrine; and it anticipates future spread and influence. Notwithstanding the prodigious amount of past and current exposition, of comment, of devastating criticism, and of defense of Freud and Freudism, the present volume has succeeded in making a notable and useful addition in all these directions. If Freud had anywhere written as straightforward and coherent an account of his beliefs regarding the soul of man as Jastrow has in his expository sections, a great amount of time, much printer's ink, and many weak souls might have been saved.

Men have usually written of Freud either as disciple and panegyrist or else as annihilating critic. Jastrow, whose view of the distempered mind has itself been strongly influenced by Freud, displays "a partial and reserved approval of principle, a profound distrust of execution." Jastrow does not "commend the greatness of a great delusion, nor yet condemn the delusion utterly, when he considers the merit of the venture that runs through its intention and formulation" (p. 142). The reader who rests with the author's statement that psychoanalysis "is an amazing conglomerate of unsupported conclusions and unnatural assumptions carried out with a speculative abandon close to irresponsibility—all of which makes it an outlaw in science" (p. 175) may leave the book with a quite wrong view of Jastrow. In such deliverances he is denouncing the Freudian unconscious and the Freudian emphasis upon sex; and in other contexts he is just as relentless in his pursuit of the logical absurdities involved in dream-symbolism and its interpretation, in transference, and in some other major psychoanalytical vagaries. But Jastrow also writes that "both for its promises and its enlightenments the motif of psychoanalysis will endure, and the work inaugurated by Freud marks a turning point in the history of psychology" (p. 260). There is here no real inconsistency. The informed psychologist will remember that one of Jastrow's fundamental concepts is the *Subconscious,* about which this keen critic of Freud and of fraud long ago wrote a book. The critic is therefore friendly to a 'depth psychology' of a sort and is himself accustomed (although he denounces the *Unconscious*) to appeal to the twilight regions of the mind for 'human motivation' and for a fabulous explanation of many facts in human experience. While many psychologists *von Fach* may find Jastrow's subconscious as dangerous to description and to sound understanding as the macrogonadic Unconscious of Freud and other lusty mystics, it is doubtless important for the tolerant and appreciative tone of the book under review that this sympathetic bond exists. It helps to make Jastrow fair to, and at times sympathetic with, a man and a movement

whose logic he finds intolerable. The cultish adherent of psychoanalysis, whose demand for factual evidence, for historical sense, and for authentic methods of observation, is no greater than that of the Viennese Master himself, is not likely to be greatly moved or enlightened by a leisurely tour through *The House that Freud Built*. Whether in religion, in morals, in fine art, or in medicine, the devotee always observes best and most happily with closed eyes, bowed head, and open mouth. But many younger men and women, who know something about the method of the sciences and something about the history of superstition and magic, will be usefully informed and critically balanced by Jastrow's candid and competent book.

Cornell University M. B.

The Jungle of the Mind. By EDGAR JAMES SWIFT. New York, Charles Scribner's Sons, 1931. Pp. ix, 340.

This, Swift's last book, revolves about a familiar thesis: thinking is not a natural and spontaneous process; the mind is prone to linger on the primitive level, setting mysticism and emotion before logic. The revolutionary discoveries of science in the last decades and the forced revision of scientific laws instead of emancipating have but confirmed mankind in their uncritical ways, and credulity now poses as open-mindedness. We still live largely on the perceptual level, accepting sense-experience —often deceptive and distorted—at its face value, or interpreting it in the light of prejudice and wishful expectation. Tagging phenomena with emotionally charged names continues to take precedence of the slower processes of analysis and experiment.

The sign-posts raised to point the way out of the jungle are, to be sure, neither new nor startling. Resistance to the thrill of mystic phrases, recognition of the subtle pull of personal bias, suspension of judgment till the supplementary facts are in, application of the touchstones of conformity with organized knowledge, of a reasoned economy in explanation—these all have a familiar ring to the psychologist and educator. Such is the author's wealth, however, of carefully collated and dissected cases, his picturesque diction, and sly humor, that shopworn topics take on a new slant and freshness. With its seasoned wisdom and sound scientific outlook, the volume gives promise of as great usefulness on the supplementary reading list as the earlier "Psychology and the Day's Work," or "Youth and the Race."

For many years, it appears, Swift had collected and followed up instances of superstition and emotionalized opinion in civilized man: belief in prophetic dreams, in materializations of the dead, in almanac prophecies of weather, in faith-healing, in panaceas for defective memory, in telepathy and the unconscious mind. Reference of such phenomena to occult or supernatural sources he combats, sometimes by citing experiments in which the effect is paralleled by purely natural means; sometimes by an appeal to common sense—the frequency of coincidence and the neglect of negative cases; sometimes by a gentle irony quite his own. Memoirs and confessions of hoaxes by magicians and men of science serve also to expose the absurdity of the lapse into "jungle ways."

To illustrate, the deceptive character of sense-experience is illumined by Patrick's experimentally demonstrated exclusion of flavors from the class of tastes. Instances of phenomenal memory in day-laborers, savants and hypnotized subjects are explained in terms of laboratory science, laws derived from which are offered to

replace and better the panaceas put out by quacks. The fallacies of 'telepathy' are dispelled by citing Beard's and Cumberland's early analyses, Downey's and Coover's experiments, and Pfungst's careful work on Clever Hans. The use of cues from eye and ear as well as muscle by palmist and clairvoyant is exposed. Moving out over a wider field, the scientific and adaptive treatment of the psychiatrist is set over the rule-of-thumb methods of the faith-healer. The phenomenon of 'suggestion,' and the psychically induced paralyses and anaesthesias of nervous disorders such as 'hysteria' are briefly expounded, as the simpler explanation of apparent miracle.

In Chapters VIII-XII, under such captions as "Have Human Beings Minds?" and "The Power Behind the Throne," the abuses of science as well as superstition come in for comment. The myopic logic of the behaviorist is handled with sly humor. Even the "twelve apostles" of the cult might read this section with profit if not with pleasure. Under another heading, "Ironclad Manacles," the limits of 'conditioning' as a device for race betterment are effectively exposed—if with something less than the brilliance of an Aldous Huxley.

As for Swift's appreciative critique of psychoanalysis, all but the more fanatic followers of Freud will concede its justice. The functional dynamic approach to neuroses is commended; while such biased postulates as exaggerated emphasis on sex, on repression into the unconscious, on a symbolism as "standardized as the parts of a Ford car" are rejected on the basis of later studies of war neuroses, 'shell-shock,' and the dreams of the normal. (Incidentally, in the opinion of the reviewer, Swift has misconceived the dream theory of Havelock Ellis.) There is shown to be no chasm between waking and dreaming consciousness, between ordinary and creative thinking. Throughout the book, indeed, the resolution of the mystic or aberrant into terms of the natural or psychological is a cardinal point. The emotional exaltation induced by foggy phrases and the thrill of posing as a member of a unique and 'progressive' sect are listed, on the other hand, as potent lures in winning followers for the cults and fads, to the detriment of clear thinking.

In spite of the attractive makeup of the volume, its popularized captions and wealth of anecdote and illustration, it strikes the reviewer as a peg above the level of the 'average reader,' at whom it apparently was aimed. Those of us who are not faddists in psychology, however, can commend it whole-heartedly to our students, not yet weaned from jungle ways by radio lessons and the introductory course; with the parting admonition that psychology "is not a patented remedy for making us healthy, wealthy and wise, as psychic fakers would have us believe;" yet it may "show the weak spots in the armor of those who care to fight their way out of the jungle of our primitive ancestors—and who seek for intellectual alertness, poise, and a robust common sense in a muddled world of muddled thinking."

Cornell University ELSIE MURRAY

Habits, Their Making and Unmaking. By KNIGHT DUNLAP. New York, Liveright Inc., 1932. Pp. x, 326.

Beginning with a preface in which the author claims to have flouted (sic!) the accepted principles of learning, this recent attempt to clarify the problems germane to habit formation and destruction, stresses two hitherto unemphasized learning fundamentals. These are, first, that we frequently learn to perform a particular activity by practicing something quite different, and secondly, that under many

circumstances the best way to break a bad habit is to practice it consciously—"negative practice." A lack of experimental evidence supporting these and other alleged principles is none too satisfactorily explained in terms of a wish stated in the preface "to spare the reader the technicalities which might not only weary him, but might distract him from the main points" (p. ix).

The first two chapters are devoted largely to a definition and formulation of the problems of habit and learning, and to a discussion of first principles. Considerable importance is attached to the allegation that thought processes are of fundamental importance in learning. While neglecting to state exactly what he means by "thinking," Dunlap seems almost convinced that without it learning is an impossibility. "It is not evident, though it *may possibly be true,* that we can learn without thinking" (p. 29, italics mine). There are also the usual criticisms of certain Thorndikian contentions, chiefly the "law of exercise." It is made logically evident that we do not learn to do by doing. "The responses which are employed in learning are not the responses to be learned" (p. 33).

Dunlap then develops in an interesting fashion his own criticism as well as a summary of the accepted criticisms which have been leveled at the "brain path" and "conditioned reflex" theories of learning. His arguments are well put, but their cogency is not enhanced by insinuations as to the lack of help given psychologists by researchers whose work has generally been considered important. This is particularly true of Pavlov. Concerning the psychological theories of learning Dunlap concludes that, "for the present, the only available theory of brain action in learning is that the brain does act in learning; but that the nature of its action is unknown" (p. 58). While the reviewer was a bit confused at calling the fact of the brain's activity an "available theory of learning," the conservatism of the statement was appreciated.

In the chapter on "The Process in Learning" a considerable amount of symbolism is employed to illustrate the nature of our learning reactions. The two principles referred to in the first paragraph of this review are elaborated in detail. Dunlap frequently dignifies his subject matter in no uncertain terms by descriptions of the complexity of learning and statements of the strenuous requirements which should be met by practicing psychologists.

In his discussion of "The Conditions of Efficient Learning" attention is called to an error which text book writers in the field of educational psychology have consistently committed; namely, unjustified generalizing from insufficient or partially digested facts. Particularly is this true in connection with whole-part learning, the distribution of practice periods, transfer of training, punishment or reward, etc. Educational psychologists have tried valiantly to make a little bit go a long way. There is a great volume of research materials, but when these are studied conscientiously there seems to have been a disheartening elaboration of the obvious or inconsequential. Dunlap believes that a number of the really fundamental problems are practically unrecognized, much less studied.

Certain of the later chapters strike the reviewer as being interesting but rather irrelevant. The treatment of personal and social adjustment involves too much discussion of the nature of behavior aberrations with a consequent neglect of their genesis or treatment. This is also true of the chapter on "Habits of Emotional Response." The concluding chapter, devoted to a discussion of "Learning Ability and

Intelligence," is largely a critique of current testing practices coupled with statements as to the nature of intelligence.

Chapter X, "The Breaking of Specific Bad Habits," is in many respects the most interesting, particularly for the lay reader. Great emphasis is placed upon the importance of initial negative practice for the removal of speech defects, tics, and irregular sexual habits. For one so obviously wedded to the scientific method, Dunlap makes some extreme statements concerning homosexuality, viz., "Every homosexual is a point of social contagion, a real vampire, whose 'bite' makes a vampire of his or her victim." "The practice of males impersonating women in college dramatics, for example, where it does not make the play ridiculous, has a direct connection with homosexual problems." "It should be noted that every man who shows a tendency to caress or needlessly handle boys, every woman who 'courts' young girls, should be watched" (pp. 222 f.).

It is to be regretted that the publishers, and at times the author, of this book imply that it was written with the non-professional reader's interests in mind. Any understanding of most of the theoretical materials involves a rather thorough familiarity with psychological principles. This is particularly true of Chapter V, the "Process in Learning." This seeming inability to decide upon the particular type of reader addressed, frequently gives the impression of an attempt to carry water upon both shoulders. It is true that the author does say in the preface that some readers "may find the reading easier if they begin with the seventh chapter and read the earlier chapters after reading the later ones."

Brief notes and a classified bibliography, arranged for each chapter, cover 53 pages at the end of the book. In certain fields it is far from complete.

University of Nebraska STEPHEN M. COREY

Ethical Relativity. By EDWARD WESTERMARCK. New York, Harcourt, Brace & Co., 1932. Pp. xviii, 301.

This book, like most works on ethics, is more concerned with word distinctions and logical relationships than with facts and inductive generalizations; it is more philosophical than psychological. Yet it makes some references to facts upon which the author is so well informed, and gives good accounts of moral emotions and their origin and relationships. Westermarck holds that moral judgments must be based upon facts of consciousness and are determined chiefly by emotional reactions and are, therefore, relative and cannot be validated objectively. He admits that we have a tendency to objectify our feelings and therefore to think of things that please us as good, and those that displease as bad; and that emotions are influenced by special circumstances and results, and by customs, concepts and beliefs; yet holds that the chief determinants of moral judgments are emotions. There are two moral emotions—approval and disapproval or indignation, both belonging to retributive emotions. Disapproval has had the larger part in the development of moral judgments. Offenders are punished not only on account of resentment but to make them change or reform, and to deter others from commiting the disapproved act.

It will be profitable to contrast Westermarck's philosophical treatment with the more scientific approach to the problem. If ethics is to be developed as an inductive science worthy of ranking with other inductive sciences, it is necessary to refrain from seeking absolute truth, and from dependence upon logic as an instrument of research.

From the standpoint of psychology a science of ethics might be developed by making a purely objective study of the behavior of men in relation to each other with a view to determining the generality of various acts classed as moral. The result would be a pure science of ethics somewhat resembling the science of plant life and growth. In applying such a science means would be sought for promoting the most prevalent of the fundamental forms of behavior, or those best adapted to bring about certain desired results such as increase of number and intensity of life activities. This would result in an applied science of ethics similar to that of hygiene, but broader.

A second method is open to those who do not take the extreme behaviorists view; that of a study of conscious ideas, emotions and volitions involved in acts generally regarded as moral, and checking the facts and introspection as completely as possible by means of the objective concomitants. Few men have a better preparation for making such a research than Westermarck. If he would abandon his philosophical and logical premise that a proposition is either true or false, in favor of the modern scientific efforts to determine by exact observation, measurement and calculation, what degree of truth is expressed by any given generalization, he could render great aid in developing a science of ethics. Such a product would be in accordance with our author's view of determinism as against chance or fate, but he would probably then be less sure of his claim that moral judgments must apply to persons and that intent must be considered by all enlightened people rather than the act alone.

It is well to distinguish between the moral acts of a person and acts judged as good or bad regardless of who commits them. To decrease disease, accidents, stealing, killing, etc., is the aim of intelligent men of today. It may well be that the enlightened man of tomorrow will expend most of his disapproval on acts themselves, and be indignant, not toward the individual committing them so much as toward a society which permits or produces the conditions under which they are likely to occur.

The reviewer regrets that our author has not increased his contribution to human knowledge by maintaining throughout his researches and writings the viewpoint of the inductive sciences, making as reliable generalizations as possible, instead of merely resting content with showing that subjective moral judgments cannot have universal objective validity.

Columbia University E. A. KIRKPATRICK

Industrial Psychology. By MORRIS S. VITELES. New York, W. W. Norton & Co., 1932. Pp. xviii, 652.

The book falls into three sections, the first on the foundations of industrial psychology, the second on fitting the worker to the job, and the third on maintaining fitness at work.

The section on foundations of industrial psychology goes into its background with a historical slant from the economic, the social, and the psychological standpoints. Individual differences are stressed in this section and illustrated with reference to industrial production.

The second section on fitting the worker to the job states the problem as involving the proficiency, competency, temperament, character, and interest of the applicant or employee. It is pointed out that the usefulness of intelligence tests is mainly at the lower borderline in clerical work, and at the professional level, and that therefore

tests for more specialized traits constitute the larger problem. The various methods of job analysis are reviewed with the emphasis primarily upon the psychological items in such analyses. The limitations of the interview and experimental demonstrations of its frequent shortcomings are then presented and are followed by some suggestions for improving the interview. Then follow application blanks, application letters, and the use of photographs, leading up to the unsatisfactory procedures of character analysis.

The third section, on maintaining fitness at work, begins with a consideration of accidents. After stating the seriousness of the problem, many studies are cited to make the point that individuals different in accident-proneness. The author feels that merely dismissing accidents as due to carelessness is scientifically unsound and that it is necessary to study the individual worker. We have efforts at testing susceptibility to accidents and also data on the effect of age, physical condition, nationality, fatigue, rate of production, atmospheric conditions, and illumination. A wide range of studies is summarized in this connection. Accidents in the transportation industry are considered separately with considerable emphasis on clinical study of the individual driver and investigation of the nature of accidents.

Pertaining to the matter of skill and training, learning curves are discussed in somewhat conventional fashion. The writer lays a wholesome stress upon reward rather than punishment for industrial learning in that the former not merely strengthens the tendency toward correct performance but tends to build up favorable feelings as well. The literature is reviewed on the question of whole versus part methods with special reference to motor skill. The author finally concludes that it is desirable very early in the operation to give the worker some idea of the process as a whole, although he may nevertheless practice considerably on the different parts. Similarly, on the speed versus accuracy question he concludes, after reviewing the literature, that the initial stress should be upon doing the performance correctly but as soon as possible some emphasis should be placed upon speed.

Two chapters are devoted to fatigue. Then follows a chapter on motives. Financial incentives are shown to be inadequate and the reader is left with the impression that motivation is an extremely complicated thing in industry due to a multiplicity of causes. Supervision looms rather large in the discussion. Then we have the maladjusted worker with some clinical material on typical cases. Finally, we have a brief treatment of supervision with emphasis on leadership.

The book has several outstanding features. In the first place, it is comprehensive and it at least touches upon practically every large problem in industrial psychology. In the second place, it has much more contact with the European literature in the field than any book thus far published in this country. The reader will probably have more respect for the German industrial psychologists than he had prior to reading the book. In the third place, the treatment is, on the whole, thorough. However, in many cases it is not clear whether the author is reviewing critically all the literature on a particular topic or merely striving to illustrate his point. He will frequently give six examples where two would suffice to make the point which he is stressing. This tendency possibly makes a bit for heaviness in the work.

The author rather deliberately avoids anything smacking of statistics and merely refers the reader to other works on the subject. For the technical reader this would suffice because he would immediately look up the methods elsewhere. But for the

person without such background it might have been well to lay a little more stress upon statistical methods merely to show him what the industrial psychologist is up against and to disillusion the reader as to any short-cuts to success in this field. However, the stress on the painstaking clinical methods which the author makes would tend to break up any such illusions. The reviewer would be inclined to include salesmen in the other groups when training was being discussed. Inasmuch as selling is such a tremendously important part of the whole program of psychology, the training of salesmen might be legitimately included in the present discussion—not the psychology of selling, but the mere training of the sales force. Then the question of morale, while mentioned specifically in a few places and discussed by implication in a good many others, might even to advantage have been lifted out and made a separate topic.

Ohio State University HAROLD E. BURTT

The Scientific Basis of Social Work: A Study in Family Case Work. By M. J. KARPF. New York, Columbia University Press, 1931. Pp. 424.

It is inevitable that a contribution which attempts to sketch the scientific basis of social work should reveal what psychology has made and should make available to aid in the social work processes. A glance through the index, clearly reveals the dependence of adequate social service on sound psychological procedures. As one turns page after page of this book one finds recognition of the contributions psychology has made to influence progress and scientific methodology in this field. Just as obviously does one find a running statement of our limitations, a catalogue of the missing or inadequately developed sections of our psychological systems and rather constant stimulation and prodding to undertake or to continue research in directions likely to yield usable data in the adjustment of human beings to their environment. The author is no less constructively critical regarding the use which the social worker now makes of the accumulated materials of psychology as well as of other social sciences.

The work of such a summary must have been extremely difficult, not alone because of the fragmentary nature of many of the contributions but because of the huge territories which had to be canvassed to locate material which had utilitarian value. Under the circumstances, the task is very well done. The field of biology, however, appears least adequately treated. It is intimated that "the biological point of view is likely to have a somewhat limiting if not discouraging and depressing effect on the social worker," and the major portion of the discussion of the contributions of biology is devoted to the pros and cons of eugenic and euthenic programs.

The contributions of psychology might have included greater amplification of the recent gains from studies of conditioned behavior, the psychology of suggestion, the psychology of testimony and report, and the significance of imitation, sympathy and a variety of other phenomena from the feeling and emotional fields. Karpf finds that very little has been made available by psychology for the immediate applications of the social worker to her tasks. If this be true, it merely emphasizes the importance of the task and the grievousness of her mission.

The second part of this book is devoted to a review of what knowledge social workers actually use, based on an analysis of family case work records. This is the first substantial effort to determine objectively what applications are really made in this field of the accumulated facts of the social sciences.

The third part of the book is concerned with an analysis of what knowledge social workers receive. It is instructive to note the extent to which schools of social work attempt to transmit, to the social worker in training, the practical applications of psychology. On the one hand, the meagerness of what psychology has to offer is contrasted by the large amount required to orient and to empower this type of practitioner adequately. This exposition should serve as a stimulus to organize more satisfactorily what we have accumulated and to apply ourselves to a number of pressing problems, the solution of which will make for more adequate service to the social work field. The need for this is clearly indicated in the final section of the book.

The Scientific Basis of Social Work is a challenge to the field of applied psychology. More thinking, more exploring, more adventuring on the part of the psychotechnologist is needed. If anyone has developed symptoms of self-complacency or self-admiration regarding the contributions which psychology has thus far made for a better regimentation of our lives, and wishes to be helpfully irritated and disturbed, he should read this book.

Graduate School of Jewish Social Work S. C. KOHS

Criminology. By ROBERT H. GAULT. New York, D. C. Heath & Co., 1932. Pp. ix, 461.

At last we have a criminology written by a psychologist! Though the author shows himself thoroughly familiar with the materials of the sociologist, the psychiatrist, the jurist, and the welfare worker and proclaims crime an inherently social phenomenon, he still directs his analyses to the offending individual called the criminal or potential criminal. The kind of mental organization and the potentialities which make it possible for human beings to get out of harmony with the social order are the major elaborations of the text, such questions as the social machinery for dealing with criminals and the techniques of detecting crime receiving only a secondary emphasis. Age, race, sex, physique, heredity, attitude, and mental abnormalities are classes of variables whose relationship to crime are very carefully evaluated, the usual errors in the interpretation of these relationships as a result of failure to detect faulty sampling procedures, etc., being admirably dragged to the light and avoided. In fact, the author in his caution seems at times almost to devitalize certain factors and set up a straw man to knock down. The general statement on page 227, which follows a lengthy discussion of the factor of age will illustrate such caution. However one may feel about the extent of certain dissections, it is a great relief, in view of the dogma that is rampant, to have available so exceedingly trustworthy a text.

In the discussions of the fundamental dynamic phases of human behavior one finds no mention in the work of those mechanisms stressed by the Freudians but rather an exposition of the concept of the prepotent reflex and the concept of a generalized drive to activity. The former the author seems to feel the more respectable, the latter serving as a sort of catch-all for the dynamic patterns which defy classification under the category of prepotent reflex. It does seem regrettable to the reviewer that more recognition was not given to such considerations—to use a type—as the universality of the direction (relative to the stimulus) of the varied adjustments to persistent pain. Perhaps Gault's unwillingness to focus upon the dynamics of mental organization with that slant is responsible in part for the

rather ineffective support which his chapter on the drives of the organism lends to later developments in the text. In the section on attitudes, however, though one might choose to wrangle over the definition presented, an approach does seem to be made to the parameter we have in mind.

Viewed as a text or reference book for college students, the volume is excellent. It is well captioned, clear cut in its organization, interestingly written, rich in well-chosen case material, concentrated in its content, and provided with an up-to-date bibliography on the topic of each of its chapters. Such questions, however, as the practical details of remedial procedures are touched rather lightly. Avenues of approach are pointed out, but detailed road maps are not given.

A tendency on the part of the author to give some attention to the history of the major issues, the points of view, and the institutions which he discusses has the wholesome effect of forcing the present status to appear in its proper proportions. Though led to feel encouraged over progress thus far made, one does not lose sight of the fact that as yet criminology boasts of no final solutions or perfect tools.

University of Chicago HELEN L. KOCH

Human Heredity. By ERWIN BAUR, EUGEN FISCHER, and FRITZ LENZ. 3rd Ed. Trans. by Eden and Cedar Paul. New York, The Macmillan Co., 1931. Pp. 734.

Although the science of human heredity has made great progress and a vast literature has been published since the first edition of *Human Heredity* in 1923, comparatively little has been added to the general theory of heredity during that period. Those who are familiar with the two earlier editions of this work will not be surprised to find little revision in the theoretical section, Section I, written by Baur. The author believes that even in the field of human heredity a process of stabilization is in progress, which will make unnecessary any radical revisions of future editions of the book. This is perhaps too optimistic an outlook in view of the present unsettled theories regarding the relation of genes to the inheritance of specific character traits and certain unsettled problems of sex determination.

Section II, written by Fischer, deals with racial differences in mankind. In this section additions have been made concerning various forms of bodily structures or types of constitution. The book was not written for anthropological specialists, but rather "to provide the reader with such elementary knowledge as is requisite for the understanding of the racial-hygienic inferences which can be drawn from a knowledge of the laws of inheritance." Of particular interest to the psychologist are conclusion that the cranial index is probably a Mendelian trait; that on the average high intellectual capacity is associated with heavier brains than the average of their national stock; and that there is not information to show that particular arrangements of convolutions are hereditary, a matter seemingly quite as important as brain weight.

Section III, on Morbific Hereditary Factors, by Lenz gives much recent data to prove the hereditary character of all forms of disease. Numerous family histories point definitely to the conclusion that myopia and other errors of refraction run in families as is also the case with albinism, nystagmous strabismus, and cataract. Cerebellar ataxia, tremor, Huntington's disease, deaf-mutism, stuttering and other disorders of speech are described as recessive Mendelian traits. Insanities and psychopathies including imbecility and idiocy, schizophrenia, epilepsy, manic-de-

pressive insanity, hysterical temperament, neurasthenia, psychasthenia, and homosexuality are also shown to run in families according to the law of recessive inheritance.

Chapter 10 is devoted to the origination of hereditary morbid predispositions. Such morbid predispositions imply anatomical disorders which themselves are hereditary. But since the anatomical basis of hereditary factors is of a physico-chemical nature, it must be subject to alteration by physico-chemical influences. The general term for the causative process effecting such alterations in the hereditary equipment is idiokinesis. Numerous experiments are cited to show the idiokinetic damage produced on offspring by alcohol. Not all the results are conclusive, but many experiments on fowls, guinea pigs, mice, and other animals as well as data collected from alcoholic intoxication in the human population give supporting evidence of various states of enfeeblement and various malformations in the offspring. "When such degenerate animals were paired, the morbid taint appeared in the second, and even in the third and fourth generation." Other idiokinetic causes are also considered, as lead, mercury, phosphorus, nicotin, carbon disulphide, benzol, anilin, and kindred substances. A number of substances used in medicine must also be included, as quinine, iodine, mercury, and arsenic. Idiokinetic effects have been demonstrated in the case of Roentgen rays, and syphilis. No support is given to the widely prevalent belief that malnutrition is a cause of morbific hereditary. Its influence is probably only parakinetic rather than idiokinetic.

Section IV, also by Lenz, is concerned with the methods for the study of Human Heredity; and Section V deals with the inheritance of intellectual gifts.

Family histories are presented to prove that musical talent is inherited. Statistical evidence is adduced from the writings of Galton, Wood, and Terman to show that genius is undoubtedly hereditary.

The last chapter is on psychological differences between the races of men. Here it is asserted that the differences between the various racial factors are in general less extensive than those between normal and morbid factors. The racial characteristics of the so-called Nordic, Mediterranean, and Jewish races are described.

There is a bibliography of 65 titles.

University of Montana FRANKLIN O. SMITH

Life and Beauty: A Spiritual Autobiography. By P. W. ROBERTSON. London, Edward Arnold & Co., 1931. Pp. 174.

"All theories of art are wrong. From the idea of beauty particular to any age each man of genius has differed, precisely in accordance with his genius. Beauty springs from life, is the enduring expression of its most passionate moments." Thus the author, who is a chemist as well as a connoisseur of art, opens his introduction, or 'Justification.' This information, he indicates, is obtained from the poets—and from his own experiences—rather than from the philosophers who "would twist and torture the words into a vain semblance of other reality." Robertson is apparently unacquainted with experimental psychology, and depends on theoretical and on Freudian methods and views for his criteria of the beautiful. Indeed he regards Freud "as one of the greatest of modern thinkers," and attributes to him the shattering of the illusion "that man is a perfectly rational being" (p. 159). Robertson even (when convenient) uses the methods of the Freudians against opponents, as when

he says that "the professional moralists are wrathful [at Freud's views] because their own inherent immorality is divulged" (*ibid.*). As used in the book before us, "life implies the unfolding story of human endeavor, the pulse and sway of the seasons, the heart throbbing in hatred or in love, the inestimable spirit of man, his inclination to the stars" (p. 11). Even this is vague enough, but from the autobiographical pictures, rather sparsely presented, one gets the following notion of the author's view: Every man in his life repeats the history of his race, whose fundamental passions—love and combat—are in fact not chaotic but simple and direct. From "the age of three to about nine" the child "recapitulates the mental history of his ancestors" from the primates to the modern civilized man. "After that age he begins to live on individual experience" (p. 17). The art-life, or rather the beauty in one's life, is thus early set in his hereditary background of passionate struggles, and, even to a larger degree, in the incessant conflicts of his early life. Thus are fixed fundamental emotional patterns which are repeated and elaborated in later experiences. The intellectual impulses in man (aroused by external stimuli) to explore, create, and to communicate his knowledge are constantly checked and guided by early scenes of love recurring in different concrete emotional settings in later stages of life, and are given shape and motivation by them. Thus interest in ships was found by the author to be expressions of impulses in the "Unconscious" to get back to a childhood love-scene. Anger seems to play a decidedly minor rôle. The artist's "desire to communicate his experience is probably very deeply rooted in the early social history of man and is derived possibly from the herd instinct" (p. 134).

The spectator of works of art is able to understand and feel the conflicts and satisfactions of the creator of the art in so far as they are able to arouse his own individual patterns formed in childhood with their accompanying emotions. Thus a scene may be beautiful to some and not to others, and beautiful to a man one day and not on another day. It is impossible to determine where aesthetic values end and other values begin. While art, according to the author's views, has no moral end or purpose, there is, however, an incessant conflict between intellectual and emotional adjustments. "The artist remodulates this disharmony in his Unconscious into a new order of life which is harmonious, thereby obtaining a sense of release and achieving a kind of freedom" (p. 133). Admitting that Croce's work has been stimulating to him, Robertson informs us that the Freudian view of the Unconscious led him toward "a more satisfying solution" of his problem and that he can now "look back upon the philosophy of Croce [identifying intuition with expression] with the respect that may still be held for an idol that has fallen by the wayside" (p. 129).

There is no inquiry into the possibility of similarity of inborn structures in individuals (hereditary or not) by means of which early emotional experiences may be largely similar in pattern in different individuals; nor is the question faced as to what extent the general interpretation given can be at all satisfactorily applied to numerous detailed phenomena in the several arts. There is no index. While the book is well worth a careful reading, it is not very suggestive as to experimental procedures on the problem. This for decades has, of course, been a weakness in 'studies' of art.

J. P.

Psychophysiology: III. Cerebration and Action. By LEONARD T. TROLAND. New York, D. Van Nostrand Co., 1932. Pp. xxiv, 446.

This book is the third in Troland's four volume work. It represents the author's attempt to correlate experience with events in the central nervous system and to give an account of the manner in which impulses are organized and released in the efferent paths.

A close reading of the book leaves one with the general impression that the author has done an extremely difficult task exceedingly well. The available facts regarding the nature of the specific operations in the nervous system which are correlated with specific types of experience are limited, and sometimes indefinite in meaning. Accordingly, Troland indicates at the outset that it will be necessary to make liberal use of the *method of hypothesis* in order to predict, at points where facts are lacking or equivocal in meaning, the most probable nature of cerebral function underlying the item of experience in question. Although the over-cautious scholar may criticize the work with a considerable degree of justification, on the ground that the author indulges too freely at many points in speculative reasoning, nevertheless it may be submitted in turn that a too cautious attitude may not contribute so much to the rate of progress with which the unrevealed facts at these difficult and complicated points are to be acquired as Troland has contributed. He surveys and interprets the known facts with the view of predicting the most probable direction which progress will take presently. The work may also be criticized by some on the ground that in certain instances the contributions of men which a decade ago received much attention are here assigned a less conspicuous place. However, the author is obviously carried by the recent swing to a strong dynamic emphasis.

As the sub-title indicates, the work falls into two parts, Cerebration and Action. Although the results of recent investigations have led certain writers to believe that consciousness is determined by general cerebration, or activity in all parts of the cerebral cortex, the corpus striatum and the thalamus and that the cerebral cortex serves as a "general dynamic battery for energizing processes whose specificity depends on lower nerve centers" (p. 68), Troland advances anatomical and pathological evidence in support of the opposite view, specificity of function in the cortex. He states that conscious experience is apparently determined by a process of *focal synergy*, consisting in a "detailed, dynamically compounded activity" (p. 128) in a limited part of the cerebral cortex, chiefly in the frontal area. Nerve currents converge upon the determinative area to form an "electrically unified synergy" (p. 67). Cerebration includes the establishment of *neurograms*, or afferent recording, and the conditions by which they are activated subsequently in reproduction, and in the linkage of these with *incitograms* which underlie the organization of efferent currents. Affection is intimately related to the establishment or destruction of neurograms. Pleasantness is correlated with increase in conductance in the cortex, the degree varying directly with affective intensity; unpleasantness, with decrease in conductance. With regard to the location of the center for feeling, the author does not agree with Head that this is controlled by the thalamus alone. He cites evidence to sustain the view that the thalamus and the cerebral cortex act coöperatively in determining feeling, and finds support in the work of Sherrington for inferring that the degree of cortical coöperation is greater in pleasantness than in un-

pleasantness. Imagination and thought are said to represent forms of cerebration which result from intra-cortical excitation of neurographic units or systems.

Having located hypothetically the cerebral foundations of conscious experience chiefly in the frontal regions of the cortex, Troland turns to the problem of action. It is clearly shown that all forms of experience are related to motor processes. The rôle which action plays in learning and the bearing of motivation upon action are thoroughly treated.

All forms of action are founded on *incitograms,* or incitation systems, which underlie the organization of efferent impulses. These are structurally separate from impression systems or neurograms, although necessarily linked with them. In voluntary action the incitation system is very complicated. Such systems are not limited to the cortex but include lower motor centers, especially in the cerebellum. The most unique part of the author's treatment of action, perhaps, is his view regarding the basis of an act of will. The process of "will" has proven so elusive as to fail to appear in introspective descriptions of voluntary acts. Still, Troland reckons with this process, giving an explanation of its cerebral foundation. In this undertaking, he relies chiefly on the writings of James, Ach, and Michotte. According to James, the characteristic phenomenon of will consists in "holding the idea of action before the mind" (p. 355). Ach would add that the idea of movement must be "identified with the self" (p. 356). Accordingly Troland relates the self to the kinesthetic system or "inner experience," and suggests that an act of will "consists in the process of bringing the kinesthetic image into proper relationship . . . or in register . . . with the total kinesthetic complex" (p. 357).

This work is a contribution of the greatest importance to the worker in physiological psychology. It is probably too theoretical, too controversial and too conjectural in nature to find application at the lower level of instruction in psychology, but the graduate student and professional man in psychology will find it indispensable. The author is accurate in his statements and clear in his reasoning. The work is notably free of typographical errors, the few exceptions which caught the attention of the reviewer being—"phyllogenetic" for phylogenetic (p. 311, 318), "prioceptive" for proprioceptive (p. 371), "desideratium" for desideratum (p. 402) and in the index (p. 445) the first initial for Swift should be E instead of D.

Fortunately for psychology this series of volumes on psychophysiology was completed before Dr. Troland's accidental death. However, in his death American psychology has sustained the loss of one of her most productive and thoughtful scholars. (See this JOURNAL, 44, 1932, 817.)

Wellesley College MICHAEL J. ZIGLER

Growth and Development of the Child, Part IV. Appraisement of the Child: I. Mental Status. II. Physical Status. Report of the Committee on Growth and Development, KENNETH D. BLACKFAN, Chairman. White House Conference on Child Health and Protection. New York, Century Co., 1932. Pp. xix, 344.

The fly-leaf of this book lists thirty-five authors, sixteen of whom contributed to the portion of the volume devoted to Mental Status. The importance of the book is derived both from its content, the distinction of the authors, and the auspices under which the volume was prepared. The views expressed in it are likely to dominate endowed research for the next ten years.

Since the volume was prepared on the committee plan, it is impossible to credit any one author with the writing of any particular section or chapter. The General Introduction to the volume perhaps expresses the viewpoint of the authorship more fully than any other chapter. If we may judge from this Introduction, the research of the next decade will study with equal vigor both the normal and the abnormal; especial attention will be given the pre-natal, the neo-natal, and the adolescent periods; animal experimentation will be continued; the fruits of clinical observation will not remain unnoticed; and exact statistical method will continue to be employed, though with greater recognition of its limitations and dangers. The Committee favors strongly "the type of study which follows an individual over a long period of time, making systematic tests and measurements at regular intervals, and gathering all kinds of relevant information about him." For some reason, the promise of human experimentation—clearly feasible in the study of immunity, nutrition, and parental education—remains unmentioned.

To the present reviewer, the General Introduction seems by far the most significant portion of the volume; the remaining chapters devote themselves largely to a survey of the data of child psychology. The first chapter, on "Mental Growth in Infant and Child" (apparently written by Arnold Gesell), stresses the importance of clinical judgment for the interpretation of mental measurements in infancy; and urges that "programs of infant training cannot be well undertaken apart from the safeguards, the technique, and the traditions of medicine." Clinical psychologists should have something to say about this! The next chapter—an excellent survey of "Intelligence Tests"—appears to deny the importance of clinical judgment, in its emphasis on the value of tests "taken by plan . . . , and interpreted by standards which have been freed from personal bias." This chapter rather scouts the importance of separate race- and sex-norms (a point of view which is duly contradicted in a later chapter (p. 186). The assertion, on p. 36, that it is at present "impossible to measure intelligence in terms of any absolute scale whose units are of equal value" seems to the reviewer to dismiss Thorndike's and Thurstone's extensive work rather briefly and curtly.

The next two chapters are devoted to "Mentally Superior Children," and "Inferior Mental Development," respectively. The first of these two chapters is practically identical, word for word, with Terman's chapter on "The Gifted Child" in *A Handbook of Child Psychology*. The brief chapter on "Inferior Mental Development" raises the question whether children who are ultimately inferior, do not "develop during infancy at a rate which much more closely approximates the normal curve of mental growth than would be expected from the ultimate mental inferiority." Such a phenomenon could be explained (although the author does not do so) on the supposition that early mental tests are less purely intellectual than later.

The chapters on the "Development of Motor Skills in the Preschool Years," and the "Development of Motor Skills in the Early School Years," will be appreciated by those who like every fact to be accompanied by an interesting problem yet to be investigated. The next chapter, "Motor Skills in Later Youth," appears perhaps as the most up-to-the-minute section in the book. Distinction is made between fine motor skills, gross motor activities, and so-called "basic motor capacities." The evidence seems quite clearly to favor the theory of specificity of skills.

"The Development of Language" is a brief, competent discussion of language

development up to about school age; disorders of speech are also discussed, including their relation to lateral dominance.

The brief chapter on "Parental Education for the Training of Children" emphasizes the responsibility of the family physician in parental education, but neglects entirely to consider the rôle of visiting teacher or parent-teacher organizations. The same medical bias is evident in the chapter entitled "Medical Attitudes toward the Sexual Education of Children." The chapter on "Training in Habits" wisely cautions parents both against over-training the child into a robot, and under-training him into erratic irresponsibility. Many a mother will wonder at the implications of the statement, in the chapter on "Behavior in Normal Children and Adolescents," that "much of the mental invalidism of the world is simply persistent childhood behavior . . . childhood psychology is fundamentally related to psychiatry and mental hygiene."

The difficult problem of heredity vs. environment is reviewed in the chapter on "Education and Mental Development," with a conclusion which emphasizes educational and environmental possibilities.

The chapter on "The Relationship between Socio-Economic Status and Mental Traits" discusses methods of measuring socio-economic status, and its relation to intelligence, language development, and personality traits (particularly honesty). Personality is further considered in the chapter on "Personality Tests." Various conduct-tests and self-rating scales are listed, and relationships with age, sex, intelligence, and the home, are discussed. Measures of reputation are but briefly mentioned.

There are four additional chapters in the portion of the volume devoted to "Mental Status." One of the few cases of disproportion in treatment occurs in the chapter on "The Influence of Prematurity on Mental Growth;" two cases studied in the Yale Psycho-Clinic receive almost as much space as Mohr and Bartelme's extensive statistical study based on objective mental-test criteria! The conclusion is that prematurely born children develop normally in the onset of walking, the beginning of talking, in bladder control, and in intelligence, provided that allowance is made for the period of prematurity when computing the child's chronological age. The chapter on "The Relation of Mental and Physical Development" reports negative findings in general, but seems to consider that suitable composite measures of physical development, if treated by suitable statistical technique, might reveal a relationship. The chapter, "Nutrition as a Factor Influencing Mental Development" is largely inconclusive. The chapter on "Structural Disturbances of the Central Nervous System—very brief—observes that real progress in the knowledge of birth injury requires adequate post-mortem data, which "will not be collected with any eagerness until the present tendency to regard any suggestion of birth injury as an indictment of obstetrical method is abandoned." The chapter emphasizes the great importance of neurological analysis of disabilities, rather than mere description in terms of "syndromes."

It unfortunately lies beyond the scope of the present reviewer's knowledge to discuss the second section of this volume, on Physical Status. Quite striking, however, is the insistence on the value, both clinically and scientifically, of repeated measurements of the same individual.

On the whole, the volume is, in content, exceptionally accurate and authorita-

tive. As a compendium of present information, the book will be most useful to both the general psychologist and the layman; as an indication of current trends, the book will be found invaluable by every serious student of child development.

Unfortunately the proof reading has been poorly done.

University of California HERBERT S. CONRAD

Change of Interest With Age. By EDWARD K. STRONG. Foreword by Walter R. Miles. Stanford University Press, 1931. Pp. xix, 235.

The author has given a meticulous clerical description of the responses of 2,340 men (384 engineers, 257 lawyers, 260 insurance men, 216 ministers, 411 physicians, 245 school men, 167 writers, and 400 Y.M.C.A. secretaries) in four age-groups (20-29 yr., 604; 30-39 yr., 759; 40-49 yr., 581; 50-59 yr., 396) to the 420 items on his "Vocational Interest Blank." That the results he describes have any causal relationship to age is in no sense demonstrated though Strong assumes that such is the case (pp. 20, 162). That self ratings of interests are not markedly different between men of 25 and 55 yr. within the same occupational group (p. 162) may mean: (1) that age is not a causal factor; (2) that different environmental backgrounds have obscured the influence of age (surely one cannot assume that environment has been the same or even remotely comparable for the men in any professional group born in 1872 and 1902); and (3) that self-ratings may describe the traditional pose of a group and may not indicate what the individual is actually like aside from the pose. Strong's treatment throughout assumes that the individual marking the "Interest Blank" not only knows what his attitudes are but is 100% honest in disclosing them—certainly an hazardous assumption. "The proverbial cautiousness of a banker is accompanied by less interest than the average in all forms of recklessness, whether physical, mental or moral" (p. ix). Banking procedures of the past decade or so should make psychologists skeptical of this self-evaluation.

That Strong is somewhat aware of the inadequacy of "control" in his investigation is shown by his statement: "The ideal procedure to follow in an age-interest study would be to determine the interests of a man at regular intervals throughout his life and base the results upon such records" (p. 15 and reiterated p. 163). This would, of course, be acceptable experimentation if one could control the environmental factors or determine their influence. Investigation without such control is comparable to estimating the amount of gas in a container without a knowledge of both temperature and pressure. Any determination of the influence of age upon mental processes awaits more careful experimentation, possible, than psychology is capable of at present. Medicine has found it necessary to revise many conclusions relative to the influence of age upon such objective matters as bone composition and blood pressure. These forced retractions should make psychology particularly wary.

Psychologists will have many minor criticisms of Strong's work: "As one does not long continue to like what one cannot do, it is only to be expected that a measurement of one's interests is approximately a measurement of what one can do" (p. 9). "Liking candy and disliking quinine are certainly innate qualities" (p. 9). "A decrease . . . amounting to 1.84 times the standard deviation of the difference is not what would be called a significant change" (p. 31). And so on.

Reports of investigations such as this illustrate the hazards of pioneering and emphasize the need of coöperatively projected long-time psychological studies. The

continuity of the author's analysis of "Interest Blank" data, dating from the early post war period, illustrates one aspect of these much-needed investigations.

Quenemo, Kansas MILTON B. JENSEN

The Problem of Musical Expression. By ERICH SORANTIN. Nashville, Marshall & Bruce Co., 1932. Pp. vi, 123.

Perhaps the most important problem in musical aesthetics is the question of whether or not musical configurations actually represent or "express" specific feelings. The matter has been vigorously debated in musical and philosophical circles, with none of the protagonists showing a disposition to approach the problem empirically. Sorantin's study is the first systematic analysis of an extensive range of musical compositions, with a view to determining whether or not general types of musical patterns or figures are used to "express" specific feelings.

It was discovered that the musical structure of compositions designed to express general feeling and "volitional" states actually manifested "a very striking similarity." In general, two types of configurations are distinguished; namely the melodic and the rhythmic, according as the major psychological import of the figure is based upon the melodic or the rhythmic aspects of the tonal configuration. Rhythmic patterns, generally, were found to portray rhythmic movement rather "directly," on account of the rhythmic motor activity induced or suggested by the pattern of tones. The periodic repetition of a two-tone motif or figure representative of pendular motion (swinging of bells) is an example. More symbolic is the "descending melodic line with diminuendo dynamics and ritardando tempo" which has been used by several composers to represent the act of dying.

The melodic figures or patterns are perhaps of greater interest psychologically. A rather striking difference was noted between figures expressive of pleasant and of unpleasant feelings. The latter (sadness, grief, despair) were symbolized by "figures of lamentation" in which the chief musical characteristics were "ascending intervals of iambic, rhythmical character, connected by iterative structure with the following descending minor second mainly in the form of a suspension or appoggiatura." Figures symbolic of pleasant feelings were characterized by the "interval of the fourth in connection with the distinct appearance of the purposive melodic line, that is, the intentional striving towards a certain note as goal, which is apparently significant of volitional efficacy." Major tonality, forte dynamics, staccato phrasing and accelerated tempo, frequent recurrence of figures of circular movement, and the trill were other aspects of the figures portraying this general class of feelings. In addition to these rather definite and simple feelings of grief and joy, more complex states such as longing, love, etc. were found to have characteristic types of figures.

All of these analyses were based upon careful study of diverse musical composers ranging from the admittedly "expressive" music of Wagner to the presumably "formalistic" music of Bach. Some 240 examples are cited and many of them are reproduced in the text. No definite information is given as to the instances in which the figures symbolizing a given feeling or emotion (as indicated by the text, for example) may not have conformed to the typical figures. One gains the impression from the number, range and diversity of examples given that Sorantin's thesis is definitely established, although the statistically-minded individual might inquire about the bases of the selections and the extent (say in percentages) of the conformity of a random sampling of selections to his generalizations.

In general, the study may be regarded as a most important piece of pioneering research. Sorantin, with his unusual combination of musical and philosophical and psychological training, has been able to accomplish a type of empirical investigation of the first order. The work should be of great interest and significance to psychologists because it will suggest problems and supply materials for experimental investigations of the affective aspects of musical experience, in contrast to the somewhat limited and often superficial work on special aspects of "sensitivity" done thus far.

Vanderbilt University LYLE H. LANIER

The Laboratory Mouse: Its Origin, Heredity, and Culture. By CLYDE E. KEELER. Cambridge, Harvard University Press, 1931. Pp. viii, 81.

This is a collection of scientific data on the house mouse, *Mus musculus*. Its origin is traced to Central Asia in Eocene times, from which place and time it has become widely distributed, perhaps as a stowaway in early human migrations, and later in ships among merchandise. Different varieties are described and their respective geographical distributions indicated. Folklore and early historical accounts show that the mouse was well known to the ancients from the earliest times, and that both rats and mice abounded where grain was stored. The mouse, as well as the rat, has been associated with plagues, detested, regarded as the handiwork of devils, and feared, while the cat, as the natural enemy of the mouse has been deified, as in Egypt. But these respective rôles have not been universal: mice have also been worshipped, *e.g.* in Asia Minor as early as 1200 B.C., and the mouse-worship cult was popular in the time of Alexander the Great. White mice were cultured in temples. "Early Greek and Roman physicians employed mice in their medical formulae" (p. 14), mouse-blood before Hippocrates' day having been used as a cure for warts. Galen in the second century, A.D., advocated for the cure of cataract equal parts of mouse-blood, cock's gall, and women's milk mixed and dried.

But in the Orient the mouse was generally in better favor, and both the Chinese and the Japanese showed interest in collecting and breeding different varieties. The waltzing variety has been known in China since 80 B.C. Over a hundred years ago fancy varieties of the house mouse were taken to Europe by British traders, and "only a few decades ago did muriculture spread to America."

The author devotes a chapter to the description of the various unit-characters (gene mutations) as they have been developed in the light of Mendel's law of inheritance, giving the fanciers descriptive terms, the scientific terms, and genetic formula in a full-page table. A well-illustrated and clearly written chapter is given over to normal inheritance of the mouse as now known, including a diagram of chromosomes with genes for unit-characters distributed arbitrarily among them to illustrate either genetic independence or linkage of the unit-characters. Certain abnormal features of inheritance, such as dominant lethals (or types which cannot live), and hereditary sterility, are also described and discussed. The final chapter covers in rather thorough manner the practical matters associated with the breeding of mice in laboratories. There are 21 very good half-tone pictures of different varieties of mice, and a selected bibliography of 184 titles. The omission of a good index is unfortunate, but is partly compensated for by a rather complete table of illustrations. The book is valuable to comparative psychologists.

J. P.

Acquisition of Motor Skill in Young Children. By JAMES ALLEN HICKS. University of Iowa Studies: Studies in Child Welfare, Vol. 4, No. 5. Iowa City, The University Bureau of Publ., 1931. Pp. 80.

This monograph reports the results of an investigation made during the author's incumbency as a National Research Council Fellow during the years 1927-1929.

Sixty children, aged 3-6 yr., were divided into experimental and control groups of 30 each. The learning task was to hit a moving target—a 6-in. bull's eye in a 4-ft. circular—with a standard squash ball. The angular velocity of the moving target was 0.3 ft. per sec. The target was 5 ft. from the thrower.

The practice group had an initial test of 10 throws, then one practice session of 10 throws weekly for 8 weeks, and two final test sessions—a total of 110 throws for each child. The control children threw 30 times, 10 at the beginning and 20 at the end of the 10 weeks of experimentation. Thus the controls had about one-fourth the total number of throws of the practice group.

In addition to the target a strength of arm and shoulder test, a perforation test (punching holes in a sheet of paper), and a test of tracing between two irregular lines were given.

In the target experiment 17 of the 30 children of the practice group gained and 13 lost in score from the initial to the final tests. In the control group 16 of the 30 children gained, 13 lost, and 1 showed no change in score.

The author and the writer of the foreword (G. D. Stoddard), conclude that "there were gains due to maturation in three of the four motor tests used, and specific practice could account for but little of the improvement." Stoddard believes that this work is "another link in the growing chain of evidence as to the primary rôle of physical maturation (as exemplified by time elapsed under normal environmental conditions) in the early development of complex motor skills."

The reviewer holds that such conclusions are not justified by the data printed in this monograph. The children were under experimental observation for less than 90 days; the range of ages studied was not broad enough to furnish convincing data on any question of growth, differentiation or de-differentiation; maturation, as contrasted with 'learning' is a highly questionable biological concept, and the author's implied distinction is nowhere defined or made clear; inferences drawn from the comparison of an experimental and a so-called control group, particularly under such limited training, are of doubtful validity; and finally the actual results show essentially no difference in the relative accomplishments of the two groups. The author cites this fact (pp. 38-39) by his comparisons of the initial and final critical ratios.

The target apparatus is well designed and should prove useful for still more extended studies on the problem. There is a bibliography of 41 titles.

Ohio State University SAMUEL S. RENSHAW

Accidents, Neuroses and Compensation. By JAMES H. HUDDLESON. Baltimore, Williams & Wilkins Co., 1932. Pp. ix, 256.

The author intended his book primarily for the medical profession. It evidences an imposing review of the literature in the field and some acquaintance with psychology. The stifling quotation of "authority" will scarcely appeal to psychologists. Its array of psychological theory is of marked service to psychology, first from the standpoint of compilation, and second, as an example of how the theory of a science

is often accepted as fact by those not intimately associated with that science. The complete acceptance of the "functional" explanation of disorder comes at a time when psychologists themselves (many of whom proselyted the doctrine) are looking askance at what now seems too simple an explanation. Science in general and psychology in particular may well be hesitant of so complete an avowal as that of Huddleson: "A *traumatic neurosis* may be defined most simply as a psychogenic or a nonstructural nervous disorder, shortly following a physical injury, and complicated or not by structural change in the central nervous system or elsewhere" (p. 1). Such a definition implies a disregard of the limitations of diagnostic technique. The fact that present methods have found no structural basis for the so-called "functional" disorders is scant justification for concluding that this will always be the case. Psychologists will find fault with the author's categorical treatment of behavior, clearly shown to be gradient instead. Psychosis and neurosis are so separated by definition (p. 3) and by treatment throughout the book.

The abundance and accuracy of citation are highly commendable. Whether the materials presented justify so many pages may be questioned and why the material is organized around twelve rather than fewer chapters is somewhat bewildering. That Chapter III on "Etiologies" merits the 38 pages accorded it seems particularly questionable in view of etiological discussion running through practically the entire 216 pages of the book proper. Insistence upon a common sense treatment of neuroses is an outstanding feature of the entire treatise. Perhaps the major contribution of the book is the finality with which it shows how little is actually known of neuroses, either as to etiology, diagnosis, prophylaxis, or treatment.

Quenemo, Kansas — MILTON B. JENSEN

The Genesis and Constancy of Ascendance and Submission as Personality Traits. By Sister MARY AQUINAS MCLAUGHLIN. University of Iowa Studies in Education, N.S., No. 216. Iowa City, University of Iowa Press, 1931. Pp. 95.

From a population of 444 students to whom the A-S Reaction Study had been given, the investigator selected certain cases whose scores deviated approximately 2 sigma from the mean. With the aid of this and an additional criterion of ratings a group of 12 markedly ascendant subjects and 13 markedly submissive subjects was selected for intensive study. The investigator then undertook a process of reëducation to determine to what extent the traits could be modified. "Frequent personal contacts afforded by weekly interviews gave opportunity for detecting erroneous concepts and substituting others more adequate for social adjustment."

At the close of the period of reëducation it was found by the criterion of ratings that 12 of the 13 submissive subjects had changed in the desired direction (*i.e.* had become more ascendant), whereas only 5 of the 12 ascendant cases had become more submissive. "The greater tension produced in submissive subjects because of unfavorable adjustment made them more susceptible to reëducation." The author lists certain causes of the traits in question and offers representative case-studies. She believes that a modification of the traits was brought about more rapidly "when the environmental factors operative in building up either ascendance or submission, were ascertainable."

The author assumes that an extreme degree of either trait is *ipso facto* undesirable.

Had she reported more fully upon the tension and disturbance which these traits caused in the individual subjects it would perhaps be possible to hazard an explanation for her interesting discovery that ascendance is less modifiable than submission. At present it is not clear whether ascendant subjects are merely more satisfied with themselves, or whether in an interview they are unable to yield to the counsellor and to accept her suggestions, or whether ascendance is the more "normal" phase of personality and when once developed resists modification.

Harvard University GORDON W. ALLPORT

Reflex Activity of the Spinal Cord. By R. S. CREED, D. DENNY-BROWN, J. C. ECCLES, E. G. T. LIDDELL, and C. S. SHERRINGTON. Oxford, The Clarendon Press, 1932. Pp. viii, 183.

Using the individual "motor unit" as the functional element of reflex response, the authors have been able to obtain accurate descriptions of reflex "time relations" and "reflex quantities of effect" as expressed in both mechanical and electrical changes. This motor unit consists of the axone of a nerve cell together with all the muscle-fibers which its many branches innervate. It makes possible a quantitative statement of the changes occurring in such conditions as fractionation, convergence, and occlusion which have proved extremely useful in accounting for such characteristics as facilitation and inhibition.

The book is not an account of the numerous investigations on reflexes of its authors. Rather it represents a complete summary, with a critical discussion, of all the important experimental work of the past thirty years. As indicated in the title, the book deals only with reflex responses manifest in preparations in which the intact central nervous system does not extend further headward than the corpora quadrigemina. The general object is to give a concise account of the mechanism of reflex behavior as found in the mammalian cord. Discussion of such topics as the "biological significance" of reflexes or reflexes as "items in animal behavior or conduct" have been purposely omitted. Such discussion would have materially increased the value of the book for psychologists. However, those espousing the principle of the conditioned response, or supporting the view that the complex acts in behavior are traceable to a synthesis of more elementary reflex units, will find the book extremely helpful in unraveling the physiological processes involved in supposedly simple reflex activity.

The first two chapters present the characteristics of the reflex arc and the properties of the spinal grey matter. In chapter three, the flexor reflex is used in describing such phenomena as the latent period, reflex tetanus, fractionation, convergence, occlusion, summation, refractory period, synaptic delay and after discharge. The next chapter deals with the stretch reflex and includes studies on the tension curve, the tendon jerk, action currents, lengthening and shortening reactions, and the action of red and white muscles. In a discussion of the crossed and ipselateral extension reflexes such factors as de-afferentation, blending, rebound, tetanic and single shock stimulation and latent successive induction are considered. Chapter six gives a very complete account of the phenomena observed in central inhibition. It contains accurate descriptions of the changes occurring in the central excitatory state and the central inhibitory state. A discussion of reflex coördination is given in the last chapter. It includes such topics as concealed

reflexes, types of rebound, quantitative adjustment of reflex contraction, reciprocal innervation, speed of movement, etc. In concluding remarks the authors present a general principle which seems to emerge from their analysis of simple reflexes. It is that "the quantity of contraction presents itself as the resultant commonly of two interacting antagonistic central processes, excitation and inhibition." There are two appendices, a very complete bibliography of 209 references, and an index.

University of California C. W. BROWN

A Field Study of the Chimpanzee: Observations of Chimpanzee Behavior and Environment in Western French Guinea. By HENRY W. NISSEN. Foreword by Robert M. Yerkes. Comparative Psychology Monographs, 8, 1931, (no. 1). Pp. vi, 122, 41 pl.

This account of chimpanzees in their natural habitat, the first study of its kind to be made by a psychobiologist, should prove almost as valuable to psychologists generally as to those interested primarily in animal behavior. Of necessity descriptive in emphasis, and admittedly an informational survey, the monograph nevertheless challenges many existing interpretations of anthropoid learning and social organization. It would appear, for example, that adaptation to the *Umwege* situation is not as detached from previous experience as is assumed by those who postulate the concept of insight.

The author, happily for his readers, has presented quotations from original notes and summaries of observations under the following broad but logically determined categories: social organization and physical appearance; nomadism, locomotion, and activity distribution; nests and nesting; food and feeding; and social behavior. Under the last mentioned category are included discussions of group leadership, play responses, contacts with other animals, reactions of chimpanzees to human behavior, and discussions of sound production and communication. Supplementing these observations and summaries, the reader will find interpretations which carry weight because they give résumés of the facts available and because they enjoy freedom from dogmatic assertion and anthropomorphic bias.

Since the investigator has recognized frankly the limitations of his study, he qualifies as the best critic. Undoubtedly future field studies will profit from this pioneer research and will in turn modify certain of its hypotheses. Be that as it may, for the present Nissen has presented to his colleagues an excellent account of the chimpanzee in non-laboratory situations.

University of Minnesota CHARLES BIRD

Psychiatry and Mental Health. By JOHN RATHBONE OLIVER. New York, Charles Scribner's Sons, 1932. Pp. 330.

The substance of this book was presented in the Hale Lectures in 1932 at the Western Theological Seminary with the purpose of orienting religious and social workers to the nature and peculiarities of abnormal people. The author has succeeded in producing a readable book, which may prove valuable to workers lacking psychiatric preparation. The appeal of the book as a whole is limited by the religious presuppositions of the author.

The cases appended to each chapter are clearly illustrative and not typical. The bibliography is surprisingly inadequate so far as psychiatry is concerned.

Trinity College ROBT. B. W. HUTT

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By J. P. GUILFORD, University of Nebraska

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